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AMERICAN ANTHROPOLOGIST

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AMERICAN ANTHROPOLOGIST

VOL. 55 AUGUST No. 3

EDITORIAL

WE HOPE our attempts to clarify the editorial policy of the ANTHROPOLOGIST will serve to encourage you to submit articles in keeping with that policy. They should also serve to give a clear picture of where the journal is headed and if our views are too narrow, too broad, or misdirected, we welcome guidance and counsel. In view of the broad ramifications of the field of anthropology, we are taking this opportunity to examine our responsibility toward one branch, archeology.

The ANTHROPOLOGIST recognizes a special interest in fields such as social anthropology where no specialized journals are published. Obviously in that field both technical detailed articles and those of broad scope should be given space. Such is not the situation in archeology where several general journals and a host of regional organs are to be found. It is not our intention to duplicate the coverage of these journals. But since they have their areas of specialization, gaps in coverage exist, and for these gaps the ANTHROPOLOGIST can serve as a vehicle of communication regarding current status and advances. Further, even in those fields adequately covered by a more specialized journal, there is a need for general summaries and evaluations of the significant contributions and concepts that are now emerging. The anthropologist often conceptualizes his role as a scholar as one in which he serves as an integrator of many lines of research bearing upon man and his culture. The very volume of published material, however, makes the task of reading and digesting primary sources a physical impossibility. Materials that are perhaps well known to investigators involved in that work may be only sketchily appreciated by specialists in other areas. We hope, therefore, to provide a means by which the anthropologist can keep abreast of developments in specialized fields such as archeology. A more specific role for this journal is seen in presenting the results of research where the interrelations between several geographic regions are discussed. Also of great interest and value are studies in which the interaction of normally separate fields within anthropology are considered, or the interplay with disciplines outside of the field of anthropology. However the specific varieties of articles mentioned should be regarded as suggestions, not an exhaustive enumeration of the only types wanted. In general we hope that

the archeological articles in recent and future issues will set a pattern for future guidance and indicate our sincere interest in publishing important articles in this field.

Our indication of a marked concern with the expansion of publication in fields such as archeology may raise a question as to what effect this might have on the availability of space for other specialized subdisciplines. We do need to conserve space where the presentation of data requires many charts, tables or illustrations. We therefore suggest that it might be desirable to follow the example provided by the physical anthropologists in the *American Journal of Physical Anthropology*. Extended versions of many of their articles or supplemental data in the form of tables, charts, or individual measurements are made available in microfilm or photocopy by the American Documentation Institute which is now housed in the Library of Congress. Could this not be similarly applied to statistical data, or for adequate documentation of any thesis advanced in other anthropological writings? The advantage is that many articles, perhaps unpublishable because of size, might be made available in abbreviated form. The writer need no longer leave much of his data in field notes or be concerned with ambiguity caused by compression due to space limitations. The specialist, considering further work in that field, would be able to purchase a report on the full materials at moderate cost from the American Documentation Institute. The procedure to be followed is to submit to the editors of the ANTHROPOLOGIST, along with a shortened version for publication in the journal, the full manuscript for permanent deposit in a form suitable for photographic reproduction. The latter requires clear, black typing on paper of standard size. A document number and price will be assigned by the American Documentation Institute and its availability indicated in a footnote at the time the summary or condensed article is published.

DAVID A. BAERREIS

The Application of Ecological Rules to the Racial Anthropology of the Aboriginal New World*

By MARSHALL T. NEWMAN

INTRODUCTION

CORRELATIONS between the physical characters of warm-blooded vertebrates and their environments have led zoologists to the formulation of several ecological rules (Allee & Schmidt 1951: 457, 460–472). The generally accepted interpretation of these correlations is that through natural selection, adaptive changes have occurred as the result of environmental stresses. Application of these rules to man has rarely been attempted. As far as I can determine, the first general suggestion that ecological rules were applicable to man came from Ridgeway (1908: 833). Schreider (1951: 823–824) interpreted the regular changes in human body weight/body surface area ratios in terms of two of these ecological rules. Unaware of Schreider's work, I pointed out the specific applicability of several ecological rules to man (Newman 1951: 191). In May, 1952, before the American Academy of Arts and Sciences, Coon applied these rules to man on a world-wide basis. Nevertheless the occurrence of adaptive alterations in recent man has been denied (Howells 1944: 217; Kroeber 1948: 167; Lundman 1952: 5–6). In large part this denial seems only to reflect an insistence that such human adaptation be demonstrated. It is the purpose of this paper, therefore, to test two of the best validated ecological rules on the aborigines of the New World, namely, Bergmann's (1847) and Allen's (1877). Both of these rules deal with the fostering of bodily heat retention or dissipation by respectively reducing or increasing the radiating skin surface per unit of body mass.

Bergmann's rule holds that within a single wide-ranging species of warm-blooded animal, the subspecies or races in colder climates attain greater body size than those in warmer climates. Illustrative of this rule is the distribution of body size in the American puma (Young & Goldman 1946), second only to man in the extent of its New World range. Map 1 shows that the average increase in the puma's body size with higher latitude forms a sustained gradient or cline for each New World continent. The same body size clines are demonstrated by other mammals of more restricted range. For example, the ermine of northeastern Asia and northwestern North America shows steep and sustained size clines for each continental area (Hall 1951: 88). Again the eight subspecies of the mule deer show average body size reductions from south Alaska to Lower California (Cowan 1936: 173).

* A shorter version of this paper was presented before the American Association of Physical Anthropologists, December 27, 1952. Published with the permission of the Secretary of the Smithsonian Institution.

Basic to Bergmann's rule is the principle that " . . . in otherwise similar bodies, the larger one has the smaller skin surface in proportion to mass, since volume and mass increase as the cube of the linear measurements and surface only as the square" (Allee & Schmidt 1951: 462). Greater body size, therefore, increases the body mass/body surface ratio. Chunkier and more compact body builds do the same thing. Increasing this ratio reduces body heat loss by radiation and therefore seems to be a cold climate adaptation. On the other hand, as Schreider (1951: 823) points out, " . . . the body mass /body surface ratio tends to decrease in climates which, at least during part of the year, put a stress on heat-eliminating mechanisms." In comparable fashion, small body size, or a slender build, increases the skin surface relative to mass, and by fostering body cooling in this way, appears to be a hot-climate adaptation. Although unaware of the Bergmann principle, Coon, Garn and Birdsell (1950: 36–44, fig. 1) developed the same idea and applied it to arctic- and desert-dwelling humans.

Allen's rule holds that warm-blooded animals living in cold climates have their heat radiating body surfaces further reduced by decreases in the size of their extremities and appendages. An example of this rule is seen in the reduction of ear length toward the north among the several North American species of rabbits (Allee & Schmidt 1951: 467). Comparable shortening of extremities is seen in a number of other mammals. This sort of reduction also raises the body mass/body surface ratio, and as such seems to be an additional cold-climate adaptation.

In mammals and birds there are a number of exceptions to these rules: 10 to 30 per cent for Bergmann's rule, calculated only from subspecies in the most contrasting climates of the species range (Rensch 1938: 282). Among these exceptions are occasional reversals in the body size clines, as in raccoons which show a northward decrease in size. Allen has suggested that such reversals of Bergmann's rule may correspond to transitions into a less suitable climate (Allee & Schmidt 1951: 465). It is also possible that in some cases the extension of the species range into colder climates is so recent that selection has not had time to operate. In addition to the reversals, there are some species showing no sustained size clines at all. In some cases the lack of clines is real, particularly with animals that by living in burrows and by other means escape part of the climatic extremes; in others, simply a matter of inadequate data. Yet as stated by Allee and Schmidt (1951: 465), " . . . when one recalls the many other means at hand for reducing the radiation of heat and the many other factors that serve to regulate body size, the small number of such exceptions is astonishing."

While the general validity of Bergmann's and Allen's rules seems to be recognized (cf. Mayr 1942), in practice their usefulness as interpretative tools in mammalian taxonomy is hampered by certain limitations not present in

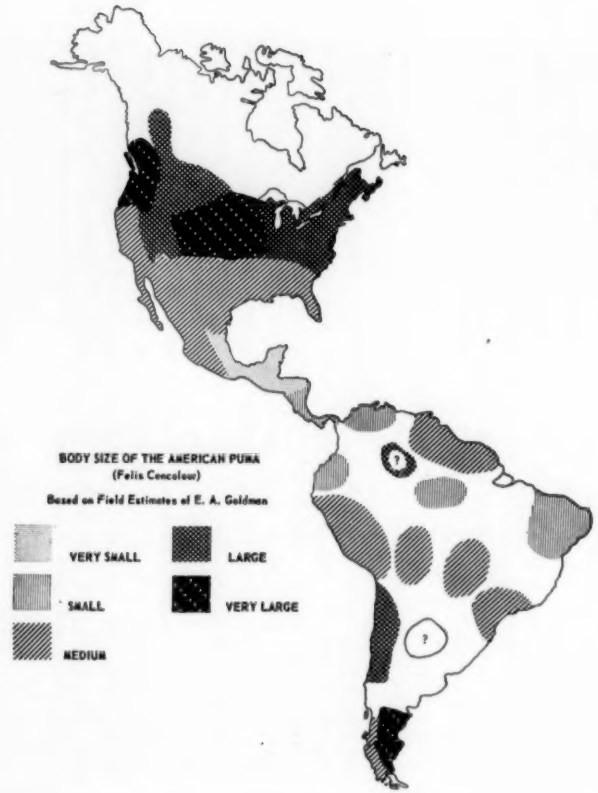
human studies. For example, in some mammals, especially the Rodentia, the small growth increments throughout life render the determination of adult body size an approximation foreign to students of man. Then, quantitative analyses of body size and proportions demand larger subspecies samples than can usually be amassed, since few taxonomists spend their lives studying one species, as anthropologists do. Moreover, most species, of mammals at least, have rather restricted ranges, which limit the gross climatic variations to which they are exposed. Thus many subspecies differences, if of an adaptive nature, are more likely attributable to local conditions, such as microclimate, than to the gross climatic variations underlying the operation of Bergmann's and Allen's rules.

In contrast, for man we have much larger series of measurements on wholly adult groups distributed over wide areas with tremendous climatic variations. In addition, the considerable body of data on post-Pleistocene human skeletons provides a third dimension usually lacking in taxonomic studies. For these reasons, it is likely that Bergmann's and Allen's rules may be more closely operative in man than in other animals. Man's assets as a test species for ecological rules need not be vitiated by the possession of a culture, since the strongly shielding effect of high culture is only 5–6000 years old anywhere, and in some parts of the world has hardly been felt.

Although testing of Bergmann's and Allen's rules is confined here to New World peoples, I am aware that in certain parts of the Old World they do not seem applicable. Upon superficial examination, the rules do not appear operative in Africa south of the Sahara. Yet, in Europe and the Near and Middle East and in East Asia and Malaysia, there seem to be north-south body size clines conforming to Bergmann's rule. The explanation of these discrepancies in Africa and perhaps elsewhere is not yet apparent.

METHOD

In the course of testing the rules, I have plotted the distribution of a number of mean male bodily and other dimensions and indices on linguistic and tribal maps of the New World (Coon, Johnson & Kluckhohn 1935; Steward 1950). Where only one series is available for a wide-spread group, I have assumed that this series is wholly representative of it. Areas for which mean figures are either lacking or wholly inadequate have been left blank on the maps. However, Biasutti (1941) and Lundman (1952), from whom I have borrowed maps 7, 9 and 10 were not so cautious, and with or without adequate data shaded in all areas. Where I have plotted skeletal measurements (maps 5, 6 and 8) I have eliminated series of considerable known antiquity, but can make no claim that these maps represent a single time level. It is more than likely that they span at least the last 1000 years. In maps 4 and 6, covering head and face size, I have simply derived the averages from the


BODY SIZE OF THE AMERICAN PUMA
(Felis Concolour)
Based on Field Estimates of E. A. Goldman
VERY SMALL
SMALL
MEDIUM
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VERY LARGE
?
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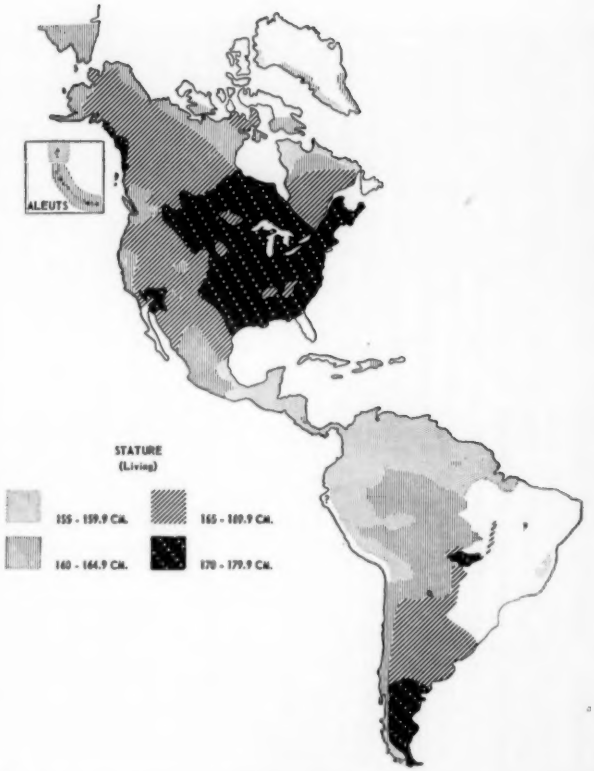
Map 1


ALEUTS
?
STATURE
(Living)
155 - 159.9 Cm.
160 - 164.9 Cm.
165 - 169.9 Cm.
170 - 179.9 Cm.

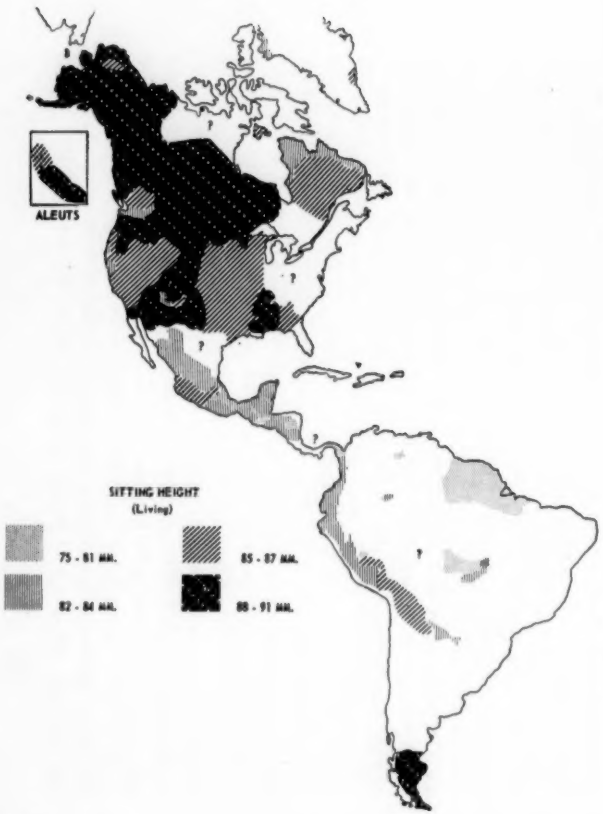
Map 2

mean dimensions. In all, the categories used in these maps are arbitrary, and in some areas the data are inadequate, but probably the maps represent reasonable approaches to the real distributions. Taken as a group, the maps tend to reinforce each other and therefore present a much more solid case than could be achieved by one map alone. This is especially the case where maps of a measurement or index on the living are confirmed by maps covering the skeletal aspects (see maps 5, 8 and 10).

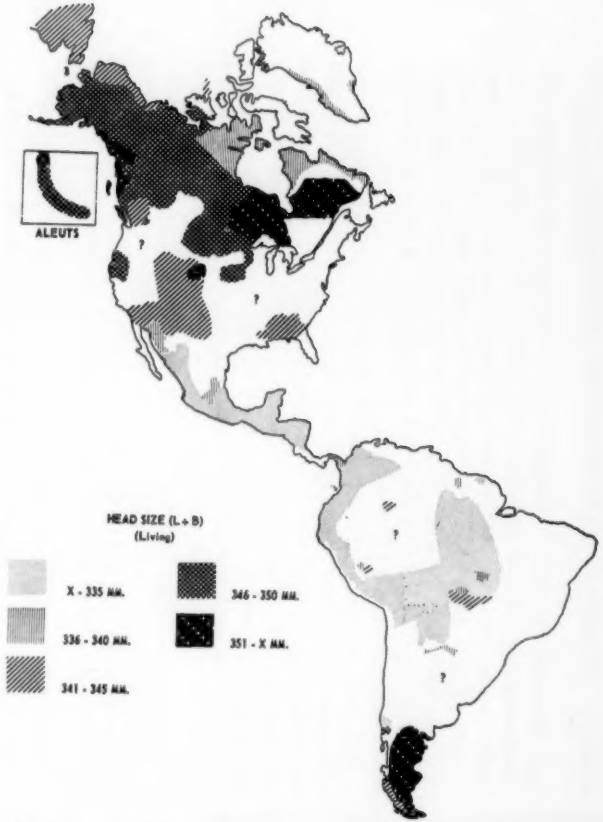
It is clear, in view of the emphasis on the body mass/body surface ratio in Bergmann's rule that a plotting of this figure would be more meaningful than linear figures. Unfortunately, individual height-weight figures for Indians and Eskimos, from which body surface may be calculated, are rather scanty and therefore do not have the Point Barrow-to-Beagle Channel distribution of the more standard measurements. And only recently were the calculation formulae found to be reasonably applicable to Eskimos (Rodahl 1952: 246). Indians have not as yet been tested in this regard. Yet this body mass/body surface approach has much merit, as Schreider (1951) is beginning to show for Old World groups, and will be needed for a more conclusive demonstration of the pertinence of Bergmann's and Allen's rules for the peoples of the Americas.

CLINES IN BODY, HEAD AND FACE SIZE IN INDIANS AND ESKIMOS

The distribution of average male stature in map 2, shows a conspicuous concentration of short peoples in the lower latitudes. To the north and south there is a progressive irregular increase in stature closely comparable to the body size clines for the puma (map 1). In American Indians the sustained nature of these stature clines is the more remarkable in view of the number and scope of well-documented intracontinental movements of people, for example: The Athabaskan push into the Southwest, the late movements into the Plains, the Tupí spread down the Brazilian coast, and the historic invasion of the Pampas by Araucanians. The regularity of the clines is also remarkable in view of the more obvious deficiencies of the basic data—inaccuracies of measurement, small size of some series and the chances of White and other admixture. In northern North America, however, the stature cline is broken by the shorter Eskimo. But as map 3 demonstrates, the Western Eskimo are not inferior in sitting height to the tallest Indians. Their shorter stature, then, is attributable solely to their short legs. This reduction of extremity length is in accordance with Allen's rule, and probably represents an adaptation fostering body-heat retention. Judging from their equally high relative-sitting-heights (Lundman 1952: map 3), the Eastern Eskimo also conform to Allen's rule, but are smaller in both stature and sitting height than are the Indians south of them. Possibly the use of heavy tailored clothing in combination with factors of uncertain food supply and periodic undernutrition may cancel out the selective advantage of larger bodies in colder climates, but this cannot



MAP 3



MAP 4

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be demonstrated. Other than in this way, I cannot account for the Eastern Eskimo's smaller body size, although in Labrador and West Greenland, Stewart (1939: 65, 68) suggests there may have been a stature decrease since European contact, due possibly to a less adequate diet occasioned by acculturation. This view is reinforced by the fact that, despite the heavy increments of White genes they have received, these particular Eskimo are still shorter than their Baffin Island and East Greenland neighbors. Other short-statured people in North America, who are surrounded by taller groups, are the Yuki of northern California and the Harrison Lake Lillouet of southern British Columbia.

The South American stature cline is broken in the extreme south by the short Yahgan and Alakaluf of the Magellanic archipelago. No sitting heights are available for these people, but other measurements indicate they are not particularly short-legged.

In map 4, head size in the living shows essentially the same sustained clines as stature, but exhibits exceptions in the Eastern Eskimo and along the southern Chilean coast. Since head size as used here is simply a length+breadth calculation, the vertical height is ignored. Thus the low-headed peoples of western North America (Stewart 1940 fig. 3) and northeastern South America (Stewart 1943: fig. 2) are actually smaller in head size than map 4 indicates, and the higher-headed peoples of the rest of the Americas are somewhat larger-headed. These discrepancies are largely resolved by map 5, which shows the distribution of the mean cranial module of the skull, a measure including the height of the vault.

The upper-face size of the skull, as expressed by the sum of the upper-face height and bizygomatic breadth, shows the same clinal distributions as head size. On map 6, large faces characterize the northern and southern ends of the native range, although the Labrador Eskimo again form an exception (see above). That there is increased upper-facial height as well as breadth with latitude is shown by Lundman's (1952) map 6. In addition, for South America at least, size of the eye orbits in skulls shows indications of following the same clines (Stewart & Newman 1950: 26).

CLINES IN BODY, FACE AND NOSE PROPORTIONS IN INDIANS AND ESKIMOS

So far only body size clines have been considered, all of which appear to follow Bergmann's rule. The trunk-leg proportions of the Eskimo are in accordance with Allen's rule (see map 3 and p. 315). Confirmatory of the body-build characters of the Eskimo is Rodahl's summary (1952 table 2), giving the surface area of different parts of the body in percentage of the total calculated surface area. Forty-two male and 11 female Eskimo from Alaska, and a patently insufficient series of 7 Whites are used. Percentagewise the Eskimo have the larger skin surface of the head, trunk, and hands; the Whites for the arms, thighs, lower legs, and feet.

MAP 4

MAP 3

ALEUTS
UPPER FACE SIZE (H+B)
(Skeletal)
X - 212 MM.
212 - 220 MM.
220 - X MM.

ALEUTS
MODULE
(Skeletal)
Below 152 MM.
Above 152 MM.

In facial proportions of the living, map 7 shows the relative elongation of the face with higher latitude. These clines are confirmed by the upper facial index on skulls (map 8).

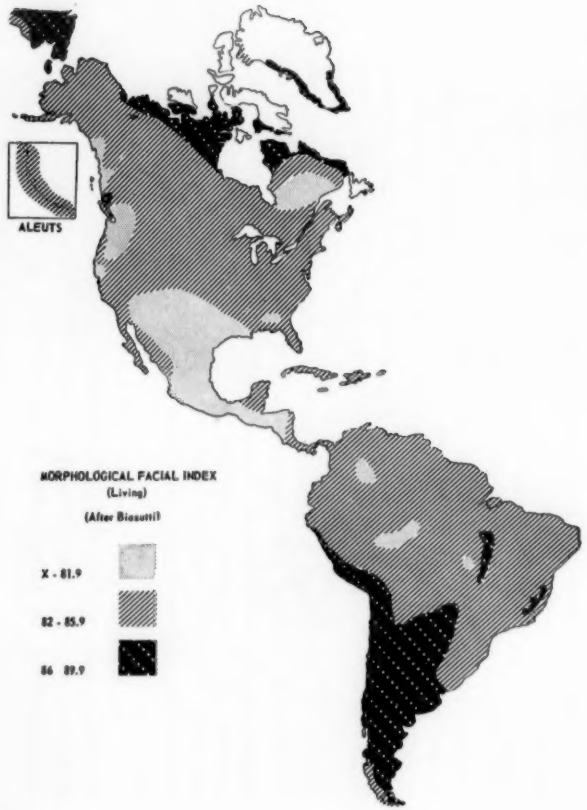
Doubtless related to the progressive elongation of the face is the narrowing of the nose with latitude first pointed out by Thomson (1913). In addition, Davies (1932: 349) has offered the unverified explanation that narrow noses represent an adaptation to cold climates where the temperature of the air must be adjusted to a warmth and humidity suitable to the lung tissue, just as a broad nose is of value in providing maximum egress for heated air from the lungs in climates hot enough to render optimum heat dissipation a selective advantage. The distribution of the nasal index on the living is shown in map 9, where the regular and sustained quality of the clines is remarkable in view of the technical difficulties in the location of nasion. Some, at least, of the five patches of higher indices between California and Central Brazil can probably be attributed to overly low location of nasion in the series reported from there (Newman 1953: 131). The clines for nasal indices on the living are paralleled by those on the skull, judging by map 10.

THE BIOLOGICAL NATURE OF THE CLINES IN BODY SIZE AND PROPORTIONS IN AMERICAN ABORIGINES

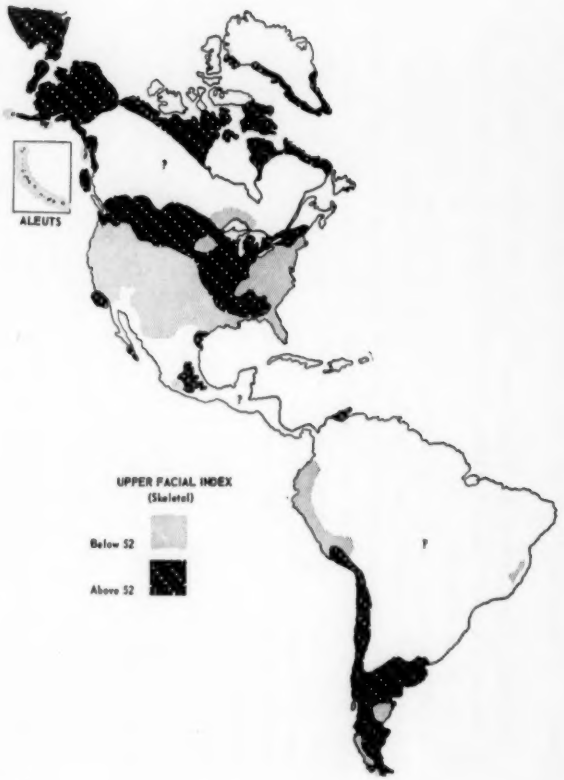
The sustained clines in body size and proportions are, in my opinion, due to adaptive changes that took place largely since the New World was first peopled about 15,000 years ago (Willey 1951: 3). In their pattern of adaptive change the body size clines seem to follow Bergmann's rule. While the body proportions of the Eskimo probably follow Allen's rule, the clines in facial and nasal proportions are not directly attributable to either rule, although in all likelihood they are due to the same factors of temperature and humidity.

Although gross climatic effect is undoubtedly important in controlling body size and proportions, there are almost certainly other factors. Possibly indications of the presence of such other factors are provided by the exceptions to the sustained nature of the clines: The smaller body size of the Eastern Eskimo, Canoe Indians, Yuki, and Harrison Lake Lillouet. Assuming the samples to be adequate, which is questionable in the last two mentioned, these peoples may be smaller because of inadequacies in food supply and nutrition. Another sort of exception is provided by the Indians living in high altitude cold along the Andes, who are small in body size though relatively large in chest dimensions, and give all indications of a separate problem in adaptation from those considered here.

In the American tropics, and perhaps elsewhere, it seems likely that food supply and nutrition may be as important as temperature and humidity in determining body size. Although we know less about human nutritional requirements in the tropics than in any other broad climatic zone, there


ALEUTS
MORPHOLOGICAL FACIAL INDEX
(Living)
(After Biasutti)
X - 81.9
82 - 85.9
86 89.9

MAP 7


ALEUTS
UPPER FACIAL INDEX
(Skeletal)
Below 52
Above 52

MAP 8

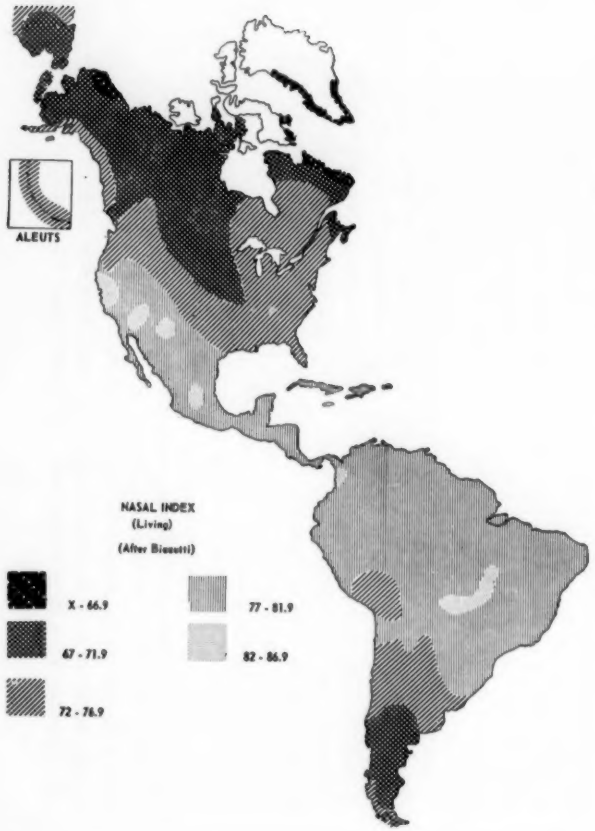
are several indications of extra stresses in that area. Mills (1941: 525) contends that the thiamine requirements of experimental animals are increased in hot environments, but whether this holds for man is not wholly certain. Again, there are indications that due to higher excretory and other losses, the iron and possibly the calcium requirements of tropic-dwelling man are also heightened (Mitchell & Edman 1951: 94). Against these and other possibilities of higher nutritional requirements for man in the tropics, there is Mills' further contention that tropic-raised meats are lower in thiamine content than those raised in temperate climates (Mills 1942: 5). If these factors stand up under the scrutiny of controlled assays and other experiments, it is apparent that humans residing in the tropics and subsisting upon locally raised foods are faced with a real dietary dilemma lending selective value to smaller body size.

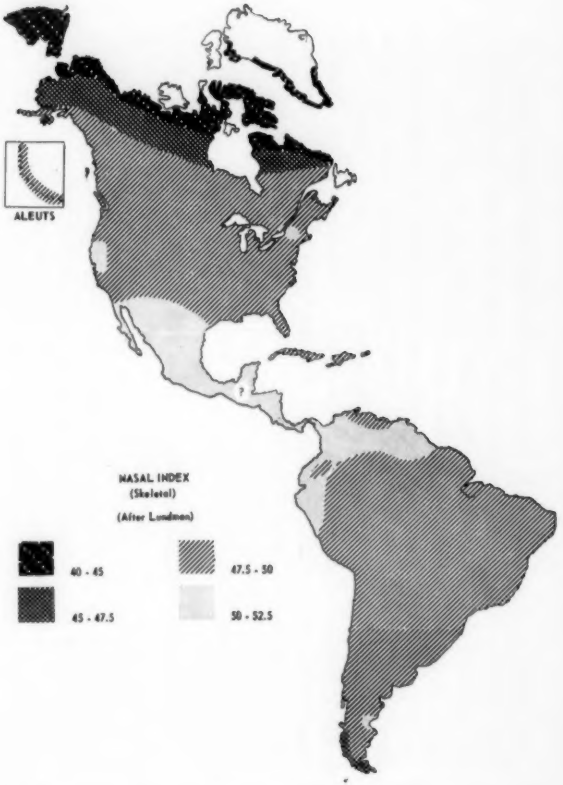
The extent to which adaptations in body size and proportions are inherited cannot as yet be determined. There are at present only general indications as to the hereditary nature of body build in man (Davenport 1923: 153). These indications are supported by evidence from other animals. As noted by Rensch (1938: 282), when several subspecies are placed in a common environment, without intermixture, there is no apparent convergence in physical characters over a number of generations. He also cites instances where body-size clines do not correspond to recent climatic zones, but rather to those of the preceding geological period. Finally he notes that there is a higher rate of cell division in the larger-bodied subspecies than in the smaller-bodied ones. Another sort of evidence favoring the inherited nature of body build is provided by production in the laboratory of large-bodied and small-bodied races of mice by pedigree selection for eight generations (MacArthur & Chiasson 1945).

MAP 8

On the other hand, there is convincing evidence that changes in the environment can have a direct non-genic effect on human body build. Lasker (1946: 274–277) has summarized the cases where there are definite but limited physical changes that took place in the descendants of immigrants: Europeans, Japanese, Chinese and Mexicans in the United States, Japanese in Hawaii, and North Americans in Panama. Particularly pertinent to the present study are Mills' (1942: 6–9) findings that, age for age, Panama-born North American children are slightly shorter in stature and lighter in weight than those just arrived from the United States. He attributes the retardation of the Panama-born to the growth depression caused by difficulties in dissipation of body heat. If so, the growth depression may be viewed as a non-genic adaptation to a body mass/body surface ratio more favorable to hot-climate living. This point is illustrated by Robinson's (1942) determination that in terms of standard physiological criteria, such as pulse rate, small men showed greater physical endurance than large men when both were placed at hard labor under

MAP 7


ALEUTS
NASAL INDEX
(Living)
(After Biasutti)
X - 66.9
67 - 71.9
72 - 76.9
77 - 81.9
82 - 86.9


ALEUTS
NASAL INDEX
(Skeletal)
(After Londmon)
40 - 45
45 - 47.5
47.5 - 50
50 - 52.5

MAP 10

conditions of difficult heat dissipation. Baker's (1953) recent findings that White and Negro soldiers lost body fat under conditions of desert stress, may be interpreted as a rapid somatic adjustment to the problem of body heat loss.

Animal experiments strongly support the evidence for man that the environment can have a direct, non-genic effect on body build. For example, white mice raised at about 65°F have significantly longer and heavier bodies and shorter extremities and appendages than those from the same stock raised at 90°F (Ogle 1934; Mills 1939). The same findings, without the increase in body length, also apply to chicks (Allee & Lutherman 1940). Thus the Bergmann and Allen effects are easily duplicated in the laboratory.

From the foregoing, it seems clear that body build is influenced by both hereditary and direct environmental factors. It is likely that the same environmental pressures that provide a selective screen favoring the better adapted body types, also make a somatic impression on the individual during his life span. As Angel (1952: 261) has pointed out, through the mechanism of relative growth, both factors—natural selection of hereditary phenotypes and direct non-genic effect of the environment—can produce striking alterations in body size and proportions. The relative potencies of these two factors in man, then, is a problem for future research.

THE SIGNIFICANCE OF THE CLINES IN BODY SIZE AND PROPORTIONS IN NEW WORLD RACIAL STUDIES

Whatever the relative potencies of the factors producing the clines shown in maps 2–10, the clines themselves are based upon most of the standard dimensions usually taken by anthropometrists. Otherwise there would have been insufficient data to plot the clines. Of the remaining standard dimensions, only head form and relative head height show distributional patterns not readily interpreted as adaptive ones. Indeed, the earlier and marginal distribution of long heads (Dixon 1923: 404) and the apparently late arrival of low heads (Stewart 1940: 43–45; 1943: 151–152) seems best explained by migrations of peoples differing in these regards. But since the diagnostic criteria of most racial classifications of New World aborigines are principally the body size and proportion traits shown here to be adaptive, it is most curious that if explanations of these classifications are attempted at all, they are in terms of a separate migration from Asia to account for each race (Stewart & Newman 1950: 28–30). Even Neumann (1952) used 4 to 6 migrations to account for his 8 North American varieties although he leaned more heavily upon morphological characters in defining his racial varieties than upon the metric features considered here. The extent to which his diagnostic morphological characters are influenced by adaptive changes in head, face, and nose size and proportions remains to be determined.

This extreme hereditarianism of most classifiers makes no allowance for physical change in the Americas except by interbreeding. Without denying that the New World was peopled by successive migrations or infiltrations of physically differing peoples, it is very likely that the American races of the classifiers are at least partly the products of adaptive changes that took place in the New World. This view does not necessarily contest any reality the classifiers' American races may have, since ecological races have definite validity in biology. Rather, it questions the classifiers' insistence that phenotypic traits involving body size and proportions could survive unaltered the vicissitudes of varying environmental pressures for 80 to 600 generations (i.e., 2,000 to 15,000 years) in the New World. It is much more probable that the sharpness of metric resemblance to Asiatic peoples fades with the number of generations in the Americas. Changes in the Asiatic parent- and the American daughter-populations, occasioned by mutation, random genetic drift, adaptation, and intermixture, would certainly blur the original closeness of phenotypic resemblance. On this basis it seems likely that the traditional metric analyses have only short range applicability in space and time to problems of racial affiliations. Effective coverage of the longer ranges may come only from morphological studies using traits whose functional and adaptive natures are understood, and, as Birdsell (1952: 358) has suggested, from controlled analyses of the genetically more complicated characters.

SUMMARY

The adaptive responses of bodily form to environment in warm-blooded animals have led, largely in the last century, to the formulation of several ecological rules. These rules constitute today an important and developing aspect of modern systematic studies, but only in the last several years have been recognized as possibly operative in man. Extensive testing of these rules on human materials, however, has not been performed. For this reason, I have examined the applicability of two of the best validated of these rules—Bergmann's and Allen's—to the body forms of New World aborigines.

The principle behind both rules is that the maximum retention of body heat in cold climates occurs when the radiating skin surface is small relative to body mass. Since this ratio can be achieved by larger body size, Bergmann's rule holds that within a wide-ranging species, the subspecies in colder climates attain greater size than those in warmer climates. Allen's rule holds that in addition the cold climate subspecies have reduced extremities and appendages, thus further reducing the body surface. In warmer climates, following Bergmann's rule, easier dissipation of body heat goes with the low body mass/body surface ratio achieved by smaller body size.

Using stature as a measure of body size, map 2 shows a concentration of short peoples in the lower latitudes astride the equator, bearing out part

of Bergmann's rule. Then stature shows somewhat irregular increases toward the north and south. These gradients or clines are sustained up to the northern continental fringe where the Eskimo show shorter statures due largely to their shorter legs. This shortness of extremities conforms to Allen's rule. Other measures indicative of body size (head size, maps 4 and 5; face size, map 6) show clines confirming those for stature.

In addition to these body size clines, maps 7 through 10 show comparable patterns of distribution in facial and nasal proportions. Both the face and nose become relatively longer and narrower with increasing latitude in North and South America.

It is important to note that these changes in body form corroborate each other, and together form a distributional pattern too closely associated with gross climatic variations to be fortuitous. That this distributional pattern in body form follows Bergmann's and Allen's rules indicates that we are dealing with adaptive changes. How much of the adaptations is due to natural selection of inherited body forms, and how much to direct non-genic effect during individual life span cannot now be determined. It seems fairly certain, however, that both factors operate in concert.

In all likelihood these adaptive changes took place since the New World was first peopled about 15,000 years ago. Now the dimensions upon which these changes are based constitute most of the traditionally taken measurements on the anthropometric blank. These same measurements are also the principal criteria used in most classifications of New World races. If the classifiers explain these races at all, they do so in terms of a separate migration from Asia for each one. This explanation seems erroneous in view of the high probability that the racial criteria themselves are of an adaptive nature. It is more likely that the races of the classifiers are at least partly ecological races formed in the New World.

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1952 VIKING FUND MEDALIST
AMERICAN ANTHROPOLOGICAL ASSOCIATION

JULIAN H. STEWARD
University of Illinois

The American Anthropological Association's medalist for 1952 is one of those remarkab individuals who, by combining doing and thinking, hasten the progress of a science. Dr. Stew ard's theoretical contributions, creative and seminal, are increasingly influential. His origin analysis of the simplest forms of family and residence groups was almost at once added to ou short list of classics; his pioneering work on cultural evolution is central to anthropologic theory. These contributions are firmly based on intensive field experience among Indians i western North America, Peru, and the Gran Chaco, and in rural and urban communities i Puerto Rico.

The Award Committee selected Steward primarily because of his achievement in editin the *Handbook of South American Indians*, which involved not only a magnificent conceptio but ability to coordinate individualistic scholars. Success here, combined with development o Steward's "area approach" led to his study of Puerto Rico, possibly an equally significant tea enterprise.

Anthropologists value Julian Steward also as an active leader in the Association, a spoke man for the profession in councils outside, and as one who has joined together scholars of a nations.

Born in Washington, D. C., in 1902, Steward studied at Cornell and California (Ph.D 1929), and taught at Michigan (1928–30), Utah (1930–33), California (1934), and Columb (1946–52). From 1935 to 1946 he did research at the Smithsonian Institution, first in th Bureau of American Ethnology and then as first director of the Institute of Social Anthr pology. He is now Research Professor of Anthropology at the University of Illinois.

ALFONSO CASO
Instituto Indigenista Nacional

Medalist chosen by the Society for American Archaeology for the excellence of his research in the ancient civilizations of Mexico, his influence on standards of archaeological research in Mexico, and the development of productive international scholarly relations.

WILLIAM L. STRAUS, JR.
Johns Hopkins University

Medalist chosen by the American Assoc ation of Physical Anthropologists for h extensive research on primate anatomy an his stimulating theories on human evolutio fields in which he has consistently produc useful facts and original interpretations.

Culture and World View: A Method of Analysis Applied to Rural Russia[1]

By D. B. SHIMKIN and PEDRO SANJUAN

THIS paper analyzes the cultures and proverbs of three rural districts in pre-revolutionary Russia. It seeks to identify the attitudes and psychodynamic features most prominent in these regions, and to examine the interrelations of these attitudes and features with other cultural segments. We chose the *genre* of proverbs for this specific goal because proverbs, a cultural element common not only throughout European Russia but also most of the Old World, offer the possibility of wide cross-cultural comparisons. Also, the varied subject matter and terseness of Russian proverbs facilitate sampling a rather wide cultural spectrum. Ubiquitous in Russian folk speech, and held by the peasant to be a guide to accepted behavior,[2] proverbs are more likely to represent the viewpoints and expressions current in given communities than individual inventions or preferences. Furthermore, the absence of organic ties between different proverbs and of strictly prescribed forms permit constant modifications in content and style (Sokolov: 258–280). Therefore, proverbs are more apt to reflect local, living attitudes and psychodynamically expressive features than other, more rigid *genres*. Finally, proverb collections of statistically useable size (over 300), presented in structurally accurate (though phonetically approximate) renderings, are available from a fair number of localities.

As a guide to the world view of Russian peasants, proverbs are subject to three limitations. First, they are obviously biased toward the common viewpoint of the adult males. The women's viewpoints emerge more clearly in other *genres*, e.g., *častuški* (a type of social song), wedding laments, and lullabies. For such sub-groups as the professional story tellers and the beggars the pertinent special *genres* must be studied (esp. Sokolov: 371–498). Second, political and moral censorship have suppressed the peasant's fondness for obscenity, and his attitudes toward the Crown, the established church, and religious sectarianism to varying degrees in different proverb collections. (Andreyev gives some data on the pre-revolutionary political censorship of folklore in Russia.) Third, since no reliable evidence on the locally-accepted, implicit connotations of proverbs has been found, the analysis has dealt solely with *explicit* meaning.

CULTURAL FEATURES OF THE THREE REGIONS

The proverbs analyzed came from Kadnikov Uyezd (59° N 40° E, east of Vologda) in 1857, Kholmogory and Pinega Uyezdy (64° 15′ N 41° 40′ E to 64° 40′ N 43° 25′ E, southeast of Arkhangel'sk) in 1864–69, and the Saguny freehold in Ostrogozhsk Uyezd (50° 35′ N 39° 30′ E. south of Voronezh) in 1903 (Popov; Yefimenko 1877–78; Yakovlev). These districts provide a

considerable dispersion both in space and time; hence, a certain sampling of the variety of Russian peasant cultures.

The principal cultural traits common to the three regions at or close to the times when the proverbs were taken appear to have been the following: all had sedentary populations primarily dependent upon the cultivation of grain, with animal draft-power used for plowing and harrowing, but with sickles and flails being the tools for reaping and threshing. The yields were low (×5 to 6 for rye, about ×3.5 for oats), and very uncertain, because of frequent killing frosts and insufficient sunshine in the north, and droughts in the south. Livestock, largely cattle and sheep, were of secondary importance. In contrast, from 20 to 40% of the gross incomes of each region came from the cash earnings of workers migrating temporarily to other areas (*otxožïye promïslï*), or hired by large landowners. Wherever the men were thus absent, the entire load of agricultural work fell upon women, children, and the aged.

Another fundamental common feature was a strong system of customary law. The origins of this law go back in general to the Kievan period and earlier, though they were considerably influenced by governmental acts in the Muscovy and Imperial eras. After the emancipation of 1861, local customary law, insofar as it did not conflict with the statutes, was officially recognized as the legal basis for the social relations of the peasant class (*krestyanskoye soslov'e*) (See Sergeyevich and Boisman). It viewed social relations, including those within the family, essentially as a network of implicit or explicit contracts, with every participant having definite rights and responsibilities, particularly in regard to property. Title, usufruct, and equity were clearly discerned concepts. Thus, dowries and inherited property belonged inalienably to the designated male or female, though their usufruct might pass in varying degree to the head of the household. Each household was, above all, a cooperative work group, with a joint capital of real property. However, the equity of every member in this work group was clearly recognized, and translated into allotments and inheritance rights on the appropriate occasions. In reflection of this principle, non-contributing household members (such as soldiers serving their twenty-five year term) often lost all rights, while totally unrelated persons working *without wage* became, at times, exclusive heirs. The jurisdiction of social pressure and of the peasant courts could enter even into spouse and parent-child relationships, but the viewpoint was almost always that of civil law, requiring a personal complaint for action, and awarding damages to the injured party. Sworn testimony and, to a lesser degree, deposited documents served for evidence; in the absence of sufficient evidence, damages were not awarded. Responsibilities were individual, not collective.

Basic social structures in all three areas were the household, the bilateral kindred, the *obščina*, and the peasant courts. Everywhere, the commonest form of household was the nuclear family. Furthermore, even in the north,

where the fraternal joint family was the ideal, each nuclear family tried to occupy a separate *izba* (winter living-quarters).

The parents' authority over the children was reinforced by religious sanctions: every major step in life needed a parental blessing, while disaster always followed a parental curse. Also, the peasant courts severely punished the abuse of parents. Child rearing was the mother's task; physical punishment, a rare and unpopular action, was the father's exclusive and sole responsibility. For the first six to twelve months, the child remained swaddled during sleep and many waking hours.[3] It lay in a swinging cradle, which was rocked to soothe or lull the infant. In all three regions, breast feeding continued for a year and a half, if the mother had sufficient milk, but this was often lacking during the summer period of hard, prolonged agricultural work. At that time too, children often had to be left under the casual care of siblings or elderly women. Because of these facts, abysmal sanitation, and inadequate clothing (a shirt being the toddler's garb), infant mortality within the first year approximated 40 to 50%; within the first five years, 55 to 60%. Parents were supposed not to mourn the loss of young children, who would go immediately to heaven.

In adolescence, both sexes had significant economic responsibilities, but social life reached a peak. Particularly during the winter months, unchaperoned evening gatherings for spinning, singing, dancing and "necking" were customary. Young men visited unmarried girls sleeping in hay lofts in the summer, or would even steal into family *izbï*. Though fickleness could lead a girl into bad repute, an illegitimate child might even enhance her desirability as one proven fertile. However, an illegitimate boy gained inheritance rights only from his mother and was therefore usually landless.

Marriage, though sometimes initiated by the young couple, was primarily an economic contract between the heads of households. The size of the dowry or the bride price (depending upon the region), age, health, strength, and industriousness were considerations. However, the landlord could dictate the marriages of his serfs (within canonical age and exogamic limits). Both patrilocal and matrilocal marriage were practised, the latter resulting from son-in-law adoption, wherein parents without sons contracted that the groom support them for the remainder of their lives and give them decent burial. In return, the young man became heir to their land, and gained usufruct of their other property. The marriage ritual was the social, emotional, and economic climax of Russian peasant life; the dowry or bride price alone generally equalling in value the entire household income for half a year. The ceremony, lasting several days, began with the selection of a go-between by the groom's father, and a formal visit of the two to the bride's parents, when friendship was sealed by bread, salt, and vodka. Economic negotiations, with the go-between acting as a legal witness, followed. The agreement was then sealed by the bride's parents (or their surrogates) who blessed the young couple. Preparations for

the wedding proper were dominated by tearful laments sung by the bride and her maids, who mourned their parting and her loss of freedom. A young, unmarried brother or male cousin formally removed the bride's maiden headdress. After the wedding came a procession of the participants, relatives, bridesmaids and groomsmen through the village. This was followed by extended revelry, with great license. Throughout, dramatic fictions were prominent: the bride *must* mourn her departure; the bride and groom are termed royalty; and elaborate courtesies are mandatory. Magic chased away or deceived evil, strengthened bonds between the young couple, united the bride to her husband's family, and promoted her fertility and that of her new household's cattle (Sokolov: 203–223).

Russian kinship terminologies delimit a bilateral group coinciding with the exogamic bounds set by Greek Orthodox canon: four "links" for consanguineal kin, and two "links" for affinal. (Only the term *dya'dina*, FBrWi, MBrWi, extends further.) The terminology is completely isolating, except that no distinction is made, among consanguineal kin, between male and female lines of descent; cousin terms derive from sibling terms; gender suffixes alone distinguish the sexes among consanguineal kin of descending generations, and among affinal kin (except DH and SOWi); and DH and SiH are merged. Within the kindred, the patterns of behavior other than exogamy were determined by the specific co-residence patterns of each household.

The heads of the peasant households belonging to one village or to the same landlord therein (*šelo*), an elected elder, and, occasionally, an appointed tax collector, formed an *obščina*. It was both the basic organ of peasant self-rule, and the bottom step of state and landlord administration. As the former, its main functions were economic aid to the unfortunate; police and fire protection; the admittance of persons to, or their discharge from, the community; and—to a varying extent—the hire of communal herdsmen, the regulation of hunting seasons, and the periodic reallotment of cultivated land. As the latter, the *obščina* had collective responsibility for payments and labor services to the government and the landlord, the provision of recruits for the army, the maintenance of public roads and buildings, etc.

Prior to 1861, peasant courts had no formal juridical basis; but thereafter they became the major function of the *volost'*, a territorial grouping of villages totalling between 300 and 2,000 males in population. These courts consisted of at least three peasant judges elected annually at a meeting of *volost'* representatives. They had jurisdiction over peasant affairs, and proceeded in accordance with local customary law (Boisman I: 28–32).

From the Greek Orthodox faith, Russian peasant cultures received many moral values, such as the cardinal importance of love, the respect due parents, the obligation to give alms, and the abhorrence of suicide. Within the household, the veneration of ikons, prayers and blessings by the elders on important

occasions, and the rigorous maintenance of fasts served as the main expressions of religious devotion. Communally, religious processions on saints' days were most prominent. Christening and burial in consecrated ground were deemed indispensable for salvation. Nevertheless, while religion was a major force, the priest played an insignificant role in peasant life. Also, beliefs in devils, in various types of household and nature spirits, and in magic remained strong. Protective incantations and soothsaying (particularly by women, at New Year's) were the main forms of socially-accepted magic; in contrast, the peasant feared cursing and blasphemy as evil, dangerous invitations to the Devil.

Within this framework of common culture, the major peculiarities of each of the three regions examined will be summarized in succession.

Kadnikov Uyezd had been colonized from Novgorod by the twelfth century; by 1628, the peasants, once free, had all become serfs bound to absentee landlords. The population tripled between this time and 1834, but failed to grow thereafter; for the entire Uyezd it totalled some 140,000, or 10 persons per square kilometer, scattered in hamlets averaging only 88 inhabitants. The increasing shortage of arable land (which had largely been taken by 1628) forced the growth of migratory wage work and cottage industries. By 1877 a third of the males older than 10 engaged in the former, while the two types of enterprise together contributed a quarter of the region's gross income. A handful of merchants dominated the cottage industries. In addition, the landlords' exactions (in the form of rental payments, or *obrok*) were rising mercilessly, climbing in one locality from 1.15 rubles per male in 1817 to 10 rubles in 1844, then levelling at 9 rubles between 1861 and 1883. The last figure constituted nearly 30% of the *gross* per capita income of this Uyezd in 1877 (data from F. A. Arsenyev, K. K. Arsenyev 13: 878–879, Ivanitskii, Mertsalov, Popov, and Shustikov).

Despite fair meadowlands and numbers of livestock (1.6 horses, 3.7 cows, and 4.7 sheep per household in 1877), inadequate plowland and fallow (less than a hectare per capita) and ruinous payments to landlords imposed constant undernutrition. The basic foods were rye bread and barley-oats-wheat loaves, supplemented by some eggs and vegetables; the principal beverages, ordinary and bread beer (*kvas*), and vodka for festive occasions. Spruce juice and similar famine foods were in regular use, yet the peasants would not eat rabbits, roosters or other "unclean" food. Gluttony was despised. Small, chimneyless ("black") *izbī* with basement stables characterized the housing. Typhus and malaria were endemic. Although the birth rate (49 per 1,000) exceeded the death rate (40 per 1,000), the emigration of males siphoned off increases, and led to a sex ratio of 100 males to 111 females.

The average size of the household, 6.5 in 1877, was large, especially in view of the shortage of males, and indicated an appreciable frequency of fraternal

joint families. The position of the women, who allegedly lacked souls, was very difficult. Men regarded wife-beating as indispensable, even praiseworthy; a henpecked husband was mercilessly mocked. Yet here, unlike other areas, the men assisted at childbirth. Although dowries and wedding costs were an immense burden, old maids suffered the most bitter scorn. Again, while a woman's barrenness was shameful, the peasants also dreaded large families, especially of girls.

Some special mechanisms of social solidarity existed. Participants at a wedding became fictional relatives termed *svatov'e* and *svatya*, with mutual obligations of friendship, including the sponsorship of weddings for orphans. Men who exchanged crosses became fictional brothers. Yet aggression within the society was marked. Rivals for sexual favors often fought with rocks, while urchins openly mocked the aged, and grown villagers sang scurrilous songs about neighboring settlements. Nevertheless, few would insult or even turn away the numerous beggars.

A marked dualism, with God and the devil being thought of as brothers; an intense fear of devils and werewolf wizards, and a high development of protective incantations with Christianized invocations characterized beliefs. While other spirits were maleficent, household spirits varied in temperament, often being kind and playful. Traces of a cult of the dead also existed: after going to church, one was supposed to visit the cemetery to bow to deceased relatives.

In the Kholmogory-Pinega region, the principal village, Kholmogory, was founded in the fourteenth century as a Novgorod monastic settlement. It flourished briefly in the sixteenth century, when English and Dutch traders made it their headquarters, but the rise of Arkhangel'sk forced it back into somnolence. In contrast to Kadnikov, this entire region remained free from serfdom; however, its strong tradition of sectarianism (*raskol*) led to recurrent conflicts with the government (data from Anisimova, K. K. Arsenyev 33: 623–4, 37: 519–21, and Yefimenko 1873, 1877/78, 1878, and 1900).

In this area, there was only 0.25 hectare of cultivated land per capita in 1899. Though average yields approximated those of Central Russia (being especially high for barley and potatoes), unseasonable frosts made harvests uncertain. Despite the abundance of fine meadows, the people kept few livestock—an average of 0.9 horses, 2.0 cows, and 2.6 sheep per household. The reasons for this may have been the difficulty of curing enough hay, and building enough sheltered barn-space for the long, severe winters. Supplementary pursuits were wage work in other regions (especially lumbering, and fishing on the White Sea); in 1899 a third of the *entire* population over 10, including many women, absented themselves periodically for such work. Hunting, particularly for squirrels, occupied about 10% of the men. Fishing in interior waters was important for subsistence but not for cash. In all, the average, annual, gross

income per capita here may not have exceeded 10 rubles in the late nineteenth century. It was, say, two-thirds of that at Kadnikov; however, the absence of severe taxation approximately equalized net incomes in the two areas.

The greatest dietary shortage among the Kholmogory-Pinega peasants was in breadstuffs, which had to be largely imported. But their diet was rich in dairy products (consumed, rather than sold, as at Kadnikov), fish, wild fowl, berries and mushrooms. Weak and strong bread beer (*kvas, xl'ebnoyevino*) were the principal beverages. Sweets were rare delicacies. The avid use or the rigid avoidance of tobacco differentiated the orthodox from the sectarians.

Although the average population density in this region was 1.2 per square kilometer, almost all of the 66,600 inhabitants (1899) lived in hamlets strung along the major streams, and averaging only 80 inhabitants each. The sex ratio here was even more skewed than at Kadnikov: 100 males to 115 females. Household size in 1864–69 average 6.2 persons, indicating the presence of many fraternal joint families here also. But in contrast to Kadnikov, the houses in the Kholmogory-Pinega region were well built and commodious, generally with two "white" (chimneyed) *izbĭ* in the front of the ground floor; and a separate barn in the rear, with a hay loft above.

The people here exhibited a strong sense of individual and property rights. Thus, women's dowries (mostly clothing), which averaged 30 to 50 rubles in value in the 1860's, passed through the female line. Their size was a major contractual issue in marriage, yet their seizure by the husband or his family met quick redress and punishment by peasant courts. If the wife died childless, the dowry had to be returned to her kin. Even a widower had no right to expend the dowry for his children, but had to hold it in trust until they came of age.

Partly because of a strong sectarian minority, family worship was exceptionally important in this region. All peasants washed and prayed both before and after each meal; the sectarians also worshipped each time they entered their own homes. Another impact of sectarian viewpoints was a great interest in the written word. These people were almost universally literate, though they confined themselves largely to religious works in Old Church Slavic. Fewer of the orthodox majority could read, yet oral readings not only of religious writings but also of traditional epics were popular among them. The sectarians isolated themselves from the orthodox in many ways, yet often sought orthodox wives. Furthermore, they married in orthodox churches, and with the customary ceremonies, which included many pagan elements. A wizard (*vĭžlivĭy*) was often employed to provide magical protection for the wedding. Also, an orphaned girl and her bridesmaids had to visit her parents' graves, and ask their permission to marry.

The orthodox population performed a series of communal ceremonies, especially at Mardi Gras and Easter. In addition, mummers dressed in animal

skins played a major role in the Christmas festivities. The sectarians, however, greatly disapproved of such mummery. Other beliefs corresponded to those at Kadnikov.

The Saguny freehold (*sloboda*) represented a distinctive enclave of 3,000 persons within the surrounding Ostrogozhsk Uyezd. It had been founded about the middle of the eighteenth century by Cossacks from Kursk and Khar'kov. Unlike many other such settlements, it remained free both of large landowners and of serfdom. Furthermore, it received its first school as early as 1842. Growing literacy may well have been a factor in linguistic Russification: by 1903, the south Russian dialect predominated over Ukrainian in this locality. The construction of the nearby Moscow-Rostov railroad toward the close of the nineteenth century also promoted commercialization. Agriculture was increasingly supplemented by trade in poultry and eggs. Bought cloth supplanted homespun, and the use of soap and perfumes grew popular. Yet many aspects of culture resisted change. A bride price of 30 to 100 rubles persisted, while the marriage ceremony retained its old complexities.

Ostrogozhsk Uyezd is, in its entirety, a black-earth, steppe country of watered valleys and dry uplands, strikingly different from the swampy, forested northern regions discussed earlier. The Uyezd was colonized only in the eighteenth century; even then, its frontier position necessitated settlement in large, compact villages. However, most of the land rapidly passed into great holdings, which required ample labor for the profitable working of the fertile soil. In consequence, serfdom here began to resemble slavery. Not only did the landowners require heavy quotas of work (*barščina*) rather than cash payments (*obrok*), but they also intervened far more in peasant life than in the north. They often shuffled the land allotments of working couples (*tyaglŭ*), dictated marriages, dispensed summary justice, and even bought and sold peasants. The emancipation of the serfs in 1861 improved conditions only moderately. True, the compulsory land allotments, which averaged 2.15 hectares of land (1.6 plowed) per capita, were sufficient to provide subsistence. But the extreme shortage of pasture, and the necessity of using straw for fuel and thatching in this treeless region, permitted the maintenance of few livestock. At the turn of the century 40% of the peasant households lacked horses. Thus, the absence of draft power often limited farming. In addition, the concentration of population, with settlements averaging 600 inhabitants, led to a dispersal of fields and pastures over many miles. Access to them could often be gained only across large estates, and at a stiff price. In consequence, possibly 10% of the households could not work their land at all; of the total gross income of all peasants, nearly 40% or some 12 rubles per capita annually had to come from wage work on large estates. Cottage industries remained negligible.

The diet consisted primarily of bread, potatoes and milk pudding, with eggs and apples as supplements. The absence of woods made berries and mush-

rooms rare; meat was eaten here, as elsewhere, only on great holidays. The typical house was log, chimneyless, with a thatched roof; in consequence, fires presented a constant, major problem, magnified by the prevalence of arson as an act of aggression. The interior consisted of one or two rooms, with the second being unheated and for summer use. Calves and lambs were quartered in the house in cold weather. Other features included separate clothing storehouses, and the general absence of bath houses (data from Pershin, Semenov, Yakovlev, Volkov, and Yefimenko 1916).

The demography of Ostrogozhsk Uyezd differed greatly from that of the northern regions. In 1879, population density averaged almost 40 per square kilometer; the average settlement approximated 600 persons and a land-use area of 27 square kilometers. Since most wage work was local, the sex ratio was equal (100:100) rather than skewed. As a result, the birth rate was even higher than in the north: 56 per thousand; the death rate was also higher (46 per thousand). Finally, the small size of the average household, 4.7 persons, reflected the complete absence of fraternal joint families. At most, there was the co-residence of aged parents with the family of their youngest son or of a son-in-law.

Because the population of Ostrogozhsk Uyezd was more than 90% Ukrainian, it is most likely that a number of the distinctive elements of Ukrainian culture were present in the Saguny freehold. If this was true, parental authority was much weaker than in the north, the individual family members retaining their own earnings, and having substantial freedom in regard to work, residence and marriage choices. The older sons received their allotments and separated from the family upon marriage; the youngest remained as the residual heir. The sex division of labor was sharp. Women engaged little in field agriculture; the household, the vegetable garden, and the poultry yard constituted their normal domain. Noteworthy too was the groom's matrilocal residence between betrothal and marriage. The Ukrainian *obščina* interfered very little in individual land use; after the emancipation, peasant sales and purchases of land became increasingly common, leading to a growing differentiation of consolidated holdings (*xutorï*), holdings under communal control, and landless peasants. The bee (*pomoč*), partnership (*tovariščistvo*), and the contracting work-group (*arl'el'*) were feebly developed in comparison with the north. On the other hand, the formal organization of young people for social festivities, and their major role in seasonal ceremonies such as the *kolyadka*, were peculiarly Ukrainian. In religion, a firm belief in the transmigration of human souls into animals, an intense fear of ghosts, vampires and were-animals, and relatively little concern for devils and nature spirits were prominent in the Ukraine.

In all, the *obrok* or cash payment system of serfdom, endemic malaria and typhus, negligible literacy, and high intrasocietal aggression marked the Kadni-

kov area. Peculiar to Kholmogory-Pinega were the absence of a tradition of serfdom or great estates; a weak agricultural base limiting population density; good diets and housing; great emphasis upon individual and property rights; and a strong sectarian tradition promoting both religious worship and literacy. Finally, the Saguny community represented an historically free, Russified enclave in an Ukrainian region dominated by *barščina*-type serfdom. High population density, large villages, small households with an absence of fraternal joint families, and an equal male to female ratio were major demographic peculiarities. In social organization important distinctions included the bride price, and (probably) weaker parental authority, a limitation of women's economic tasks, and looser communal organizations and controls in economic fields. Significant religious differences may also have existed.

ANALYSIS OF THE PROVERBS

The authors codified, compared quantitatively, and interpreted such aspects as the proverbs reveal of three socio-psychological dimensions: the visualized pattern of social relations, the philosophical orientation, and the system of logical and perceptual organization. While no rigorous validation of the results is feasible, an effort has been made to assess the congruence between them and the general cultural data summarized above.

Developing a code for the pattern of social relations and the philosophical orientation necessitated a preliminary qualitative survey of the bodies of proverbs studied, to note all the kinds of relations and values mentioned; however, only items mentioned more than five times in the three samples of proverbs jointly were retained for quantitative analysis (Figures 1 and 2). Many themes, such as the concern with the insane, the crippled, and the dishonest which is so prominent in Caucasus proverbs, are either rare or absent. In depicting the social pattern, the exact definition of the relation was also important. Consequently, the junior author coded each social category, *wherever the evidence was explicit*, in terms of "positive," "ambivalent," or "negative" evaluation; and also in terms of the concrete type of behavior, *e.g.*, dominance-submission.

The selection of items indicating logical and perceptual organization was based on adaptability to interpretation from the viewpoint of Rorschach test theory (see Hallowell). The senior author assumed that rigid control corresponded in inverse degree to the frequencies of elaborating devices (figures of speech, whimsy, etc.), adjectives, references to color (chromatically bright colors weighted at 1.5), intensity and texture; and of references to action, both actual and implied. The frequencies of references to black and white, and to speech, thought and sight, respectively, were assessed as measures of substitutive displacement in behavior or fantasy. The numbers of intensity and texture references served as guides to anxiety and sensitivity; the frequencies of meteorological and passive verbs, to inner compulsions. Leads to the degree of introversion or extroversion at various levels were taken to be the following four ratios: (1) All colors (bright ones, x1.5) plus black and white references *to* all actual and implied action verbs, as the most general index. (2) All colors (bright ones

at 1.5) *to* true action verbs, on the level of direct, habitual behavior. (3) Black and white *to* speech, thought and sight references, on the level of symbolic behavior. (4) Intensity and texture *to* passive and meteorological verbs, on the level of sporadic, impulsive behavior.

Finally, three elements may express the relative weight of depression and particularism, of matter-of-factness and common-sense, or of elation and high aspiration. These are the ratio of evaluative to descriptive adjectives (and adverbs), as a measure of high aspiration; the frequency of animal references, as one of matter-of-factness; and the frequency ratios, for human and animal references, between wholes, large parts (body, head, limbs) and small parts (eyes, ears, mouth, reproductive organs, arms, etc.), as indications of all three factors.

Illustrative of the coding procedure are the following examples, the first three of which are from Kadnikov; the second three, from Kholmogory-Pinega; and the last three, from Saguny.

1. I krasno da ńe l'eto, i dobra da ńe mat'
   Beautiful but not a summer, kind but not a mother.
   Coding: positive reference to women; positive reference to mother; mother to child, affection; descriptive and evaluative adjectives; elaborating device (contrast); whole human reference.
2. Xto xorošo govorit, tovo ńe darit', a xto i xudo govorit, tovo ńe branit'
   (Him) who speaks well (of you), do not reward, and (him) who speaks ill (of you), do not scold.
   Coding: prevents action; evaluative adverb; speech verb; human reference (whole implied).
3. Maleńkaya sobaćkya layot, ot bol'šlyo slīšīt
   A little dog barks (what) it hears from a larger one.
   Coding: Youth; descriptive adjectives; verb of speech; animals; personification of animals.
4. Guba tolšče, tak bryuxo tońše
   Thicker lips, thus a thinner belly.
   Coding: descriptive adjectives; semantic elaboration (contrast); large (belly) and small (lips) body parts.
5. Muž lyubit ženu zdorovuyu, a brat lyubit šestru bogatayu
   A husband loves a healthy wife, a brother loves a wealthy sister.
   Coding: husband-wife, affection, ambivalent; brother-sister, affection, ambivalent; woman ambivalent; love; semantic elaboration (contrast); human reference (whole implied).
6. Soldat b'ez špagi xuže babī
   A soldier without a sword is worse than a peasant woman.
   Coding: soldier, ambivalent; women, negative; semantic elaboration (whimsy); whole human reference.
7. Xoroša bīla beśeda—napiliś i podraliś
   The chat was good—they got drunk and had a fight.
   Coding: evaluative adjective; reference (verbal noun) to speech; passive verbs; semantic elaboration (whimsy); human reference (whole implied).

8. Rabota durakiv lyube
   Work loves fools.
   Coding: personalized abstraction; whole human reference.
9. Yesli sl'eva uvidiš m'yesyac v novo luńe, k nesčastyu ili ńe'udače[4]
   If you see the moon at new moon from the left, (then) accidents or bad luck (will come).
   Coding: seeing; mythological, negative.

To effect quantitative comparisons, the junior author selected 500 proverbs (on the basis of tables of random numbers), from each collection, coded them on I.B.M. cards, then tabulated by machine.

To scale explicitly evaluated items in the categories of social relations and of elemental forces, all mentions for each item (e.g., gentry) were *added*: twice this sum plus one gives the possible variance scale between extremes. If the actual evaluations are then scored as positive = +1, ambivalent = 0, and negative = −1, then their algebraic sum must fall within the variance scale, and may be rendered as a percentage of the maximum. The percentage groupings used herein follow:

Over 80% of maximum: strongly favorable.....++
60 to 79% of maximum: favorable.....+
40 to 59% of maximum: ambivalent.....±
20 to 39% of maximum: negative.....−
Under 20% of maximum: very negative.....=

To illustrate the scaling method: for one item, value mentions totalled 11. Hence the variance scale was 23, i.e. +11 to −11. Actually, 6 favorable, 3 ambivalent, and 2 unfavorable proverbs occurred; their algebraic sum is thus +4. On the scale of 23, +4 is 16 or 69% of the most favorable possible. Hence the rating was "favorable" or +.

The quantitative results are tabulated in Figures 1, 2 and 3. The interpretations of these data must be qualified by two warnings: first, that *we are dealing with social images of unknown statistical representativeness, and not directly with the "average personalities" of the three regions.* Second is *the unproven nature of the methods used, particularly the adaptation of Rorschach theory.*

The aggregate image of social relations in all three districts is marked by ambivalence and by an emphasis upon antagonism. The attitudes toward outside ethnic groups, particularly the Jewish, are everywhere negative and hostile. In contrast, soldiers universally gain approbation. Kadnikov emphasizes poverty, the clergy, the Jews, and, within the family, parent-child, child-parent, and in-law relationships. Interactions between siblings and between spouses receive very little attention. The Kadnikov proverbs stress cooperation and duty, particularly within the family, but also (concurrently with antagonism) in the dealings between social strata. In the Kholmogory-Pinega area little interest is exhibited in the clergy, gentry or outside ethnic groups. On the other hand, the popularity of, and the sense of duty toward, the Tsar are strong. The ambivalence toward poverty and the expressed dislike for wealth contrast with the opposite overt tendencies in the Kadnikov and Saguny

	Kadnikov			Kholmogory-Pinega			Saguny		
	I	II	III	I	II	III	I	II	III
FAMILY AND FRIENDS:	156	+	C, D (S, A)	81	+	L, S A, D	54	±	(S)
Parents to children	48	+	(C, D)	8	(±)	(L, A)	8	(±)	
Children to parents	52	+	(C, D)	25	++	(L, S)	8	(±)	
Between siblings	0			3	+		4		
Bride to bridegroom	7			5	(+)	(L)	10	(±)	(S)
Wife to husband	0			9	(+)	(S)	6	(±)	
Wife to parents-in-law	12	(±)	(C, D)	10	(+)	(S, A)	0		
Husband to wife	0			4	(±)		4		
Husband to parents-in-law	14	(+)	(C)	7	(−)	(A)	3	(±)	
Husband to siblings-in-law	19	±		0			1		
Between friends	4			10			10	(+)	
WOMEN	97	−		88	−		52	±	
SOCIAL STRATA	128	=	A, (C)	98	−	A	84	±	(A, S)
Peasants	3	(=)		3			9	(+)	
Poor (poverty)	38	(−)		11	±		6	(−)	
Rich (wealth, property)	31	(±)		31	−	A	17	(±)	(A)
Merchants	10	(=)	(A)	15	−	A	7		
Clergy	23	±	(A)	7	(±)		12	(−)	(A)
Gentry	9		(C)	6	(−)	A	12	(=)	
Officialdom	4			10	(=)	(A)	11	(−)	
Soldiers	10	(++)		7	(++)		7	(+)	
The Tsar	0			8	(++)	(D)	3		
ETHNIC GROUPS	44	=	A	27	=	A	43	=	A
Foreigners	2			7	(=)	(A)	7	(=)	(A)
Germans	6	(−)		3			6		
Tatars	7	(−)		0			11	(=)	(A)
Jews	23	(=)	(A)	12		(A)	7	(=)	A
Sectarians (Old Believers)	6			0			2		
Non-local Russians	0			5	(=)	(A)	10	(+)	
SUMMARY	325	+	A, C, D	294	±	A, L, D, S	233	+	A, S

Symbols: I Frequency (number of proverbs in sample of 500).
II Proverbs strongly favorable, ++; favorable, +; ambivalent, ±; negative, −; strongly negative, =.
III Themes expressed: A, antagonism; S, submission-dominance; C, cooperation; D, duty; L, affection. Items with a frequency of 3 to 9 mentions per sample of 500 are in parentheses; over 50, underscored.

FIG. 1. The patterns of social relations depicted by the proverbs.

	Kadnikov		Kholmogory-Pinega		Saguny	
	I	II	I	II	I	II
POSITIVISM TO NEGATIVISM	307	=	315	=	192	=
Proverb stimulates action *to* proverb deters action	160	=	201	=	113	−
Proverb expresses optimism *to* proverb expresses fatalism or pessimism	147	=	114	=	79	=
THE LIFE CYCLE	121	±	79	±	84	±
Birth *to* death	38	±	52	−	25	−
Youth *to* old age	83	±	17	±	40	+
Health *to* sickness	0		10	±	19	−
ELEMENTAL FORCES	48	±	76	±	95	±
God	23	++	24	++	33	(++)
Devils, spirits, etc.	14	±	31	=	36	−
Natural forces	11	−	21	+	26	±
ETHICAL AND SOCIAL ABSTRACTIONS	116	+	105	±	99	±
Love *to* hatred	30	+	38	++	32	++
Piety *to* ungodliness	16	+	4	(+)	8	(++)
Poverty *to* wealth and property	69	−	42	−	23	−
Magnanimity *to* avarice	0		7	(−)	14	±
Justice *to* injustice	1		14	+	22	−
SUMMARY	592	−	575	−	470	±

Symbols: I Frequency (number of proverbs in sample of 500).
II (a) Ratio of positive to negative items, or (b), for Elemental Forces, the degree of favorableness of the force toward man.

FIG. 2. The philosophical orientation depicted by the proverbs.

regions. An ambivalent attitude of the parents toward their children, yet a strongly positive one of the children toward their parents, is also distinctive. So too are the strong emotions disclosed: affection and submission-dominance marking most family relationships; great antagonism, those toward the outside world. At Saguny, the social image depicts extreme ambivalence in relationships within the family and between social strata, and in attitudes towards women. Submission-dominance appears to be a primary mode of behavior. Emphasis upon the various sets of social relations seems rather uniformly distributed, though spouse-in-law relationships are largely ignored. However, in comparison with Kadnikov and Kholmogory-Pinega, mentions of poverty and wealth are few.

	Kadnikov	Kholmogory-Pinega	Saguny
RIGID CONTROL			
Inversely to frequencies of:			
Devices of semantic elaboration (figures of speech, whimsy, etc.)	61	76	68
Adjectives, adverbs	255	335	206
Color, intensity and texture references (bright color at 1.5)	63	188	80
Action, speech, sight and thought references	382	340	518
DISPLACEMENT			
Directly to frequencies of:			
Black and white references	0	0	5
Speech, thought, and sight references	151	73	136
ANXIETY AND IMPULSE			
Directly to frequencies of:			
Intensity and texture references	5	38	13
Passive and meteorological verbs	47	57	38
EXTROVERSION TO INTROVERSION			
Directly to ratios of:			
Color, black and white mentions *to* action, speech, thought and sight verbs	0.16	0.55	0.15
Bright (at 1.5) plus cool colors *to* action verbs	0.25	0.56	0.16
Black and white *to* speech, thought and sight verbs	0.04	0	0
Intensity and texture *to* passive and meteorological verb references	0.10	0.66	0.34
LEVEL OF ASPIRATION			
High, *directly to ratio of:*			
Evaluative *to* descriptive adjectives	0.34	0.23	0.57
Matter-of-fact, *directly to frequency of:*			
Animal references	83	100	76
High: matter-of-fact: depressed, *directly to ratio of:*			
Whole: large part: small animal and human references	4.8:1.0:0.9	4.0:1.0:0.24	6.4:1.0:1.6

FIG. 3. Psychodynamic indications projected in the proverbs.

The philosophy of all three areas (Figure 2) is marked by negativism, fatalism, and pessimism. Yet all believe in a strongly beneficent God, in love above hatred, and in piety over ungodliness. Also, wealth and property uniformly receive more attention than poverty. Kadnikov holds that the supernatural forces other than God are ambivalent toward men, but that nature is hostile. Wealth and poverty seem to be dominant pre-occupations; mag-

nanimity, avarice, justice, and injustice are ethical problems of no concern. Also conspicuous is the absence of health and sickness themes. The Kholmogory-Pinega and Saguny regions manifest substantially common philosophies, with one exception: the former stresses justice: the latter, injustice.

The psycho-dynamic indications (Figure 3) sharply differentiate Kholmogory-Pinega and the other two areas. The first manifests less rigid psychological controls, particularly over emotional vectors: far less introversion, no evidence of emotional displacement, and little of action symbolism. However, sensitivity, anxiety and impulsiveness appear relatively strong. Matter-of-factness, as opposed to elation or depression, is characteristic. Saguny also displays some interesting features. With a degree of introversion comparable to that at Kadnikov, it exhibits more anxiety and emotional displacement and less affect. In addition, both the levels of aspiration and the depressive tendencies shown by the proverbs of this area are high.

Given these interpretations, it appears likely to us that the many stereotypes (including the oppositions between God and clergy, Tsar and officialdom) represent intellectual rigidity rather than emotional displacement. On the other hand, note the changes in the expressed behavior within families, from affection, submission-dominance, antagonism and duty at Kholmogory-Pinega; to cooperation, duty, submission-dominance, and antagonism at Kadnikov; to plain submission-dominance at Saguny. These appear to reflect extroversive-introversive differences, and the degree of their manifestation as direct affect or anxiety. Finally, the high degree of ambivalence (especially in regard to family relations) which Saguny displays seems to reflect psychodynamics prone to elation-depression.

Are these interpretations congruent with the cultures of the three regions? A categorical answer cannot be given since several kinds of attitude-culture interrelations appear. In many instances the proverbs clearly reflect the cultural conditions of the period: thus, the hardships and uncertainties of peasant life justified pessimism; man's powerlessness before authority made fatalism and a belief in God as his only chance quite rational. In more detailed matters, the Saguny stress on spouse as opposed to parent-child and in-law relationships, and the opposite elsewhere, may be correlated with the absence and presence of the fraternal joint family, and with the degree of parental authority. The status of women at Saguny was obviously better than in Kadnikov Uyezd; possibly better than in Kholmogory-Pinega. An association between attitudes toward women and the demographic sex ratios may thus exist. The crudeness of Kadnikov social attitudes seems congruent with this group's aggressiveness in speech and act. But, while overt dislike of wealth and ambivalence toward poverty at Kholmogory-Pinega appear to reflect Christian teachings among pious folk, the actual social behavior was not so lofty.

Some attitudes, such as the Saguny and, especially the Kadnikov, dislike

of Tatars, appear to be historical survivals. Some, such as the deep antipathy toward the Jews and the Kholmogory-Pinega aversion to gentry may be stereotypes imported from other areas. In this regard, the prevalence of migratory wage work suggests a possible mechanism of transfer. Several items indicate suppression, at the manifest level, of emotionally dangerous matters. Within this rubric fall the virtual absence of references to sectarians and to piety at Kholmogory-Pinega; the lack of proverbs on sickness and health in disease-ridden Kadnikov. Finally, lack of information prohibits judgment on many matters, *e.g.*, whether ambivalence in family relations was actually highest at Saguny.

Distinctively lower introversion and constriction at Kholmogory-Pinega, and high elation-depression at Saguny can neither be proven nor disproven from direct evidence other than congruence with expressed attiudes. Assuming, however, that these are real phenomena, what may be their causes? Our speculation—offered only as a lead for further study—would be to relate the first to the historical absence of serfdom and to the atmosphere of freedom in the former region. For the second, we visualize the penetration, even into a relatively favored but small enclave, of habit systems engendered by the direct interference, into daily life and personal integrity, of the *barščina* system of serfdom and its living survivals. At the same time, other possibilities, such as dietary differences, and differences in unrecorded details of child training, cannot be ignored.

CONCLUSIONS

The present study suggests an approach to the delineation of the major attitudes and psycho-dynamic patterns common to relatively homogeneous communities through a systematic analysis of proverbs. The first phase of this analysis is to code proverb content and forms for the social interrelations and philosophical orientations depicted, and for the psycho-dynamic indicators (in terms of adapted Rorschach theory) found. A preliminary, qualitative content analysis determines the specific items constituting the social and philosophical categories applicable to each body of proverbs. The second phase is one of quantitative comparison, aided by reducing all the bodies of proverbs compared to a uniform number, on the basis of a table of random numbers. Definite, uniform quantification of all descriptive terms used in the verbal interpretation forms the third phase, which includes the separate collation of the direct attitudinal materials and the psycho-dynamic indicators; also, the attempt to interrelate the two coherently. The last phase is a qualitative, verbal assessment of the degree of fit between the interpretations drawn from the proverb analysis and other pertinent cultural information.

The method described has been applied in detail to the proverbs of three linguistically Russian rural districts: Kadnikov Uyezd, Kholmogory and

Pinega Uyezdy, and Saguny Sloboda in Ostrogozhk Uyezd. These regions are considerably dispersed in space and time: northwest Russia, far-northern Russia, and the Black Earth belt; 1857, 1864–69, and 1903, respectively. But they have common economic foundations, customary law, household functions, child rearing practices, pre-marital sex license, exogamic limits, wedding customs, institutions of self-government, and Greek Orthodox beliefs. They differ, above all, in the absence, presence and type of serfdom and its survivals; in demography, especially population density, concentration, household size, and male to female ratios; in diet and housing; in the position of women; and in the character of extra-Christian religious beliefs. The materials from the proverbs indicate maximum uniformity in the strongly negative attitudes toward outside ethnic groups; in emphasis upon negativism, fatalism, and pessimism; in belief in a beneficent God; and in stress upon love and piety as values. On the psychodynamic side, the weak development of elements interpreted to be indicative of emotional displacement was the major uniformity. The greatest variations were noted in the pictures of family interrelations, in attitudes toward women, and in the degrees of emphasis upon magnanimity, avarice, justice-injustice, and health-sickness. In psycho-dynamics, the indications of extreme introversion were far less pronounced in the far-northern Kholmogory-Pinega than in the other two areas; while Saguny, the southernmost, showed considerable evidence of basic elation-depression. The fit between the interpretations and other cultural information is neither absolute nor simple; direct reflections, historical survivals, diffused stereotypes, and suppressions are all evident. A cultural basis for the imputed psycho-dynamic differences cannot be established except speculatively; the authors incline to view variations in the development of serfdom as the most likely explanation.

In general, it is believed that further experimentation with the methods outlined is worthwhile. They have obvious promise in other cultures where no possibility of direct psychological testing and social measurement exists. Even in those instances where a direct approach is possible, the methods may be of value for special purposes; above all, in distinguishing between ideal and statistical norms, and between the projections evidenced in habitual, spontaneous creations as opposed to culturally foreign introductions. Finally, the separate study and integration of data from several approaches (including the analysis of various *genres* most characteristic of sub-cultures, *e.g.*, beggars; or of individual creativity, *e.g.*, funeral laments) can, in conjunction with proverb analysis, throw considerable light upon the complex problems of culture and personality.

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NOTES

[1] Presented at the Wenner-Gren Foundation Conference of Anthropologists and Linguists, University of Indiana, July 21–30, 1952. Aid in formulating the proverb codes was received from Drs. R. A. Bauer, H. Beier, and E. Hanfmann, and Mr. D. Gleicher, all of the Russian Research Center. Two types of transliteration are used in this paper: geographical names and the bibliography follow the orthography of the National Geographic Society (1929), except that post-consonantal palatalization is denoted (by an apostrophe). Other terms are in conventional Smithsonian Institution transcription as modified in 1934, (Herzog *et al.* 1934), except that palatization is marked by a superimposed slash.

[2] The logic of the educated peasant Shustikov in confirming the rightness of mutual aid in the *obščina* by means of a proverb is typical (see Shustikov 244).

[3] Swaddling was by no means universal in peasant Russia, being absent, for example, in the Onega region of Arkhangel'sk Guberniya (Pokrovskii: 135–36).

[4] This type of literary form is usually placed in the *genre* of omens (*per'em'etš*), but is included by Yakovlev (1905–06), with proverbs.

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Archeological and Linguistic Chronology of Indo-European Groups

By MORRIS SWADESH

UNTIL recently no purely linguistic method existed for objectively gauging the antiquity of language divergence. Hence, in her coordination of archeological cultures and prehistoric linguistic groups, Marija Gimbutas (1952) had at her disposal only relative linguistic chronologies except insofar as scholars had previously attempted to do what she did, that is, to associate linguistic divergences with movements of people and to assign dates to them on the basis of archeological evidence. The most specific evidence for connecting given prehistoric stages of languages with definite archeological cultures is that of "linguistic paleontology." Thus, historic languages like Latin, Teutonic, Church Slavic, Greek, Persian, Sanskrit, which are differentiated late continuations of a single ancient prehistoric tongue known as "Indo-European," have related words for various agricultural concepts. From this it follows that the prehistoric "Indo-Europeans" already practised agriculture and raised domestic animals before they separated and scattered. As Gimbutas points out, "The main surviving names of the common speech point to a neolithic stage of development; no certain word for any metal survived" (p. 606). For relatively recent prehistoric periods, important additional evidence may be obtained from place names; just as the toponymy of modern America includes a certain number of geographic names from the Indian languages, so too the map of Europe contains linguistic vestiges pointing to the tongues spoken in each area prior to the earliest known history.

To these lines of evidence we can now add that of lexico-statistic dating, based on the recently discovered fact that the every-day, non-cultural vocabulary tends to be replaced at an approximately constant rate (Swadesh 1952). Wherever an ancient speech community has broken up, either because of migration or the weakening of such ties as intermarriage, common ceremonial events, trade, travel, etc., the linguistic changes in one area become more or less independent of those taking place elsewhere. From the percentage of cognate words still present in two test vocabularies, we can therefore obtain an index of the time period during which the two languages have been diverging. While much research still needs to be done before this method can be improved to its maximum of potential accuracy, it has already proved approximately correct in a series of cases where it was possible to check it against historical and archeological knowledge. Such instances, to which "North Indo-European" can now be added, include:

(a) English-German: Lexico-statistic date 12.5 ± 1 centuries ago (Lees n.d.). Historical date of Anglo-Saxon invasions of Britain 6th century A.D.

(b) Eskimo-Aleut: Lexico-statistic date 29 ± 4 centuries ago (Marsh and Swadesh 1951). Carbon 14 date of pre-Aleutian sites $3{,}018 \pm 230$ years ago (Libby 1952: 89).

(c) Huaxtec-Yucatec: Lexico-statistic date 32 ± 4 centuries ago (data from Norman A. McQuown and Alfredo Barrera Vasquez respectively). Archeological estimate 35 centuries ago (given author by Alfred Kidder and Gordon Willey).

To obtain the lexico-statistic date of a linguistic divergence, one sets down the equivalents in the compared languages of the series of meanings constituting the test list; the one used in the present assay consists of 200 items (Sawdesh 1952: 456–457). Each pair of languages is examined for agreements of form and meaning, that is for cases where the words for a given item of meaning are phonetically such that they are evidently derived from the same original form in the common period of the two languages. The criterion of cognate sameness is that the two forms must correspond phonetically in accordance with the transformations of sounds which are known to have taken place in each line of development from the original common language. In addition to these phonetic "laws," one also takes into account assimilatory-dissimilatory, and analogical changes and the use of symbolic mutations ("ablaut") and of affixes. The percentage of agreements in the test vocabulary becomes an index of the time of divergence when compared with the constant of retention worked out on the basis of historically controllable instances. The constant is $81\% \pm 2\%$ of retention per 1,000 years in each vocabulary. The constant of common retention for two related languages is the square of the simple constant, or 66% $\pm 3\%$ per 1,000 years. Since the basis of the method is published elsewhere, further exposition will be omitted here.

A thoroughgoing lexico-statistic study of Indo-European, covering the ancient and modern languages, has not yet been made. We offer here cognate lists (verified and corrected by André Martinet) and percentages for Russian, English and French as representatives of the Slavonic, Teutonic and Romanic branches. In these lists, resemblances due to accidental convergence and to recent borrowing are disregarded. Cognate agreements are included even where the resemblance is obscured by phonetic, analogic and morphologic changes.

For Russian-English we find the following cognate sets in the test vocabulary: ja I, dva two, trji three, chetjírje four, pjatj five, uxo ear, jestj eat, jajtsó egg, padatj fall, pjeró feather, plavatj float, sjerdtse heart, kak how, v in, znatj know, ljevij left, ljezhátj lie, dljinnij long, muzhchína man, mnogije many, matj mother, imja name, novij new, noch night, nje not, nos nose, solj salt, sjemja seed, shitj sew, korótkij short, sjidjétj sit, maljenkij small, snjeg snow, stojátj stand, sosátj suck, solntse sun, etot that, tam there, tonkij thin, etot this, jazík tongue, djerjevo tree, vodá water, chto what, kogdá when, gdje where, kto who, vjetjer wind, zholtij yellow.

For English-French: I je, one un, two deux, three trois, four quatre, five

cinq, at à, blow souffler (Latin sub-flare), come venir, ear oreille, egg oeuf, eye oeuil, father père, fear peur, few peu, fish poisson, foot pied, heart coeur, how comment, in dans, long long, mother mère, name nom, new neuf, night nuit, not ne-pas, nose nez, other autre, red rouge, righthand droit, root racine, salt sel, sand sable, sew coudre (from Latin con-suere), short court, sit s'asseoir, snow neige, star étoile, suck sucer, sun soleil, tongue langue, tooth dent, what quoi, when quand, who qui, wind vent, worm ver.

For French-Russian: je ja, tu ti, vous vi, deux dva, trois trji, quatre chetjírje, cinq pjatj, écorce korá, os kostj, jour djenj, mourir umirjátj, poussière pilj, oreille uxo, oeuf jajtsó, loin daljekó, donner datj, coeur sjerdtse, comment kak, dans v, vif zhivój, long dljinnij, mère matj, nom imja, neuf novij, nuit noch, ne-pas nje, nez nos, sel solj, mer morje, voir vjidjetj, coudre shitj, aigu ostrij, court korótkij, s'asseoir sjidjétj, neige snjeg, sucer sosátj, soleil solntse, langue jazík, quoi chto, quand kogdá, qui kto, vent vjetjer.

These agreements amount to 49 in 200 or 24.5% for Russian-English, 47 in 200 or 23.5% for English-French, 42 in 200 or 21% for French-Russian. Translating these percentages into minimum time-depths, we get 33 ± 4 centuries for Slav-Teutonic; 34 ± 4 centuries for Teuton-Romanic; and 37 ± 4 centuries for Roman-Teutonic. On the basis of geographic distributions in historic times, uncontradicted by archeologic evidence, we would suppose that the Indo-European dialects from which the three branches of the stock are derived were situated in the order Slavonic-Teutonic-Romanic, and the percentages of agreement bear this out. As the central dialect, Teutonic shows relative affinity to its two neighbors. As geographically separated dialects, Romanic and Slavonic have the least lexical agreement. Of the three time-depths, the greatest, that between the two extremes, should be the most accurate measure of the actual time, since some degree of contact between neighbors would have acted as a brake on full divergence in the other cases. The lexicostatistic date of 37 centuries ago is the one to be compared with Gimbutas' archeological determination of 2000–1800 B.C. as the time of the population movements which gave rise to Teutonic and Slavonic as separate entities.

It would be desirable to obtain Slavonic-Teutonic-Romanic lexical statistics for earlier stages of the three branches. Ephraim Cross (1952) gives such figures for Old Church Slavic, Gothic, and classical Latin, historically dated at approximately 1000 A.D., 350 A.D., and 50 B.C. respectively. His percentages, based on a different test list of 240 words, are 35% for Slavic-Gothic, 40% for Gothic-Latin, 32% for Latin-Slavic. Since the constant was calculated only on the other list and not on Cross's, the two sets of percentages cannot be regarded as exactly comparable to each other, but various instances in which agreements were measured by both lists indicate that the results run fairly close together. The time-depths, as given by Cross's percentages on the basis of the 81% constant and adjusting for the disparity of date in the three ancient languages, are 38 ± 3 centuries ago for Slav-Teutonic, 39 ± 2

centuries ago for Teuton-Romanic, and 42 ± 3 centuries for Roman-Teutonic. These dates run five centuries farther back than the other set, suggesting that there is a consistent small disparity between the two test lists. However, the difference cannot be reduced to a simple formula, because Cross's score for English-German comes out a half-century shorter than that obtained by our list. Lees (n.d.) finds 58.5%. Cross tells the writer he finds 60% with his list.

The relationship between lexical divergence and geographic separation due to migration may vary depending on the special circumstances. First, if the two groups speak essentially the same language at the outset and if the separation is complete, the vocabulary divergence should coincide with the period of time since the migration. Second, the two peoples may have spoken dialects which were already divergent at the time of separation; in this case, the lexical divergence will of course be greater than the time since migration. Or third, the two groups may continue to maintain something of their original speech community for some time after the migration, tending to slow down the rate of divergence from each other. Our figures suggest that the third situation prevailed, and the archeology supports this in its evidences of extensive trade relations.

The fact that lexical statistics can contribute to the solution of these problems of prehistory makes it highly desirable that linguists and prehistorians undertake extensive studies of the statistical facts and the causal theory of vocabulary divergence, aimed at deepening our comprehension of cultural change and contributing to the methodology of dating prehistory. As to north Indo-European lexico-statistics, although there is still an element of uncertainty in the method, they already give important confirmation to Dr. Gimbutas' theory that certain cultural and populational movements around 2000–1800 B.C. are connected with the prehistoric separation of Teutonic and Slavonic.

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Wenner-Gren Foundation Supper Conference

THE Wenner-Gren Foundation supper conferences usually held in New York are now being arranged by the editors of the Anthropologist. Several conferences in different regions will help develop material for the journal. The first conference, held in Chicago on February 21st, was based on a draft of the paper (below) by I. Schapera, who was visiting at the University of Toronto. Copies of the paper (slightly abridged) were sent for comment to D. F. Aberle, D. A. Baerreis, W. Bascom, D. Bidney, F. Eggan, C. W. M. Hart, R. Redfield, J. H. Steward, D. B. Stout, J. B. Watson, and L. White. Their comments were distributed in advance of the conference. The first session, in the afternoon, opened with an analysis by Milton Singer of the issues presented in the documents and continued with two hours of free discussion, after which Singer summarized anew the agreements reached and the remaining issues. At the second (supper) session, it was agreed that (1) Schapera would revise his paper for publication to clarify points which the discussion led him to believe were misunderstood; (2) Singer would prepare for publication his analysis of the whole discussion; while (3) the participants remained free to communicate their views further in the Anthropologist. Many suggestions for further articles, conferences, and research enriched the discussion and will see fruition in this journal.—Editor.

Some Comments on Comparative Method in Social Anthropology

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SOCIAL anthropologists are generally agreed that many of the problems interesting them involve the comparison of different societies with one another. But, if one may judge from the way in which they set about making such comparisons, there is at times little regard for method. In particular, the principles (if any) that govern the selection of societies for comparison seem to be anything but scientific; nor, in this context, is there even any consistency about what is meant by a "society," i.e. about the nature of the units between which comparison is being made. I propose to illustrate these remarks by reviewing briefly some comparative studies recently published by leading British and North American anthropologists, and, in order to contrast more effectively the different methods of approach, shall deal mainly with studies in the same general field, that of kinship and marriage. But what I have to say would apply equally to studies in economics, government, law, religion, or any of the other topics usually discussed by social anthropologists.

The anthropologists of the late 19th and early 20th centuries who dealt with kinship and marriage, such as Frazer and Westermarck, were not unduly concerned with the problem of deciding which societies to compare. They aimed at utilizing data from every known society, or at least from every known primitive society; and as a rule the only reason why any particular society might be ignored was lack of the relevant information. Moreover, and this needs to be remembered, the writers were usually not content with a single source, but consulted as many as they could find, and, where it seemed necessary, they tried to distinguish between them and to account for discrepancies or contradictions.

Hardly any of the work done more recently is of the same comprehensive character; there has been no attempt to discuss the kinship and marriage systems of every society known to us, or even to deal with every known variety of those systems. One reason almost certainly is the great increase in the number of societies about which information is now available; it has recently been stated, for instance, that "There are today some three to four thousand peoples whose cultures are sufficiently distinctive to be considered independent units." (Ford and Beach: 5. I cite these writers because they use the methods of the Cross-Cultural Survey.) It is therefore almost impossible for any one worker to digest all the literature on the subject. There has also been a change in critical standards and in the nature of the problems considered, so that many of the older sources—and often they are still the only sources on particular societies—tend to be regarded as of little value and are therefore not used.

Almost every general treatise on social anthropology—and during the past five years such treatises have been published by Firth, Gillin, Herskovits, Hoebel, Lowie, Piddington, and Turney-High, among others—contains one or more chapters on kinship and marriage. Those chapters are as a rule mainly descriptive, giving a survey of what the author regards as the principal varieties of marriage forms and arrangements, family types, systems of kinship terminology, kinship usages, unilineal kinship groups, and so on. In addition, there is sometimes special discussion of topics held to be of theoretical importance. In each case, specific reference is made to a number of different societies, perhaps thirty or forty, of which half a dozen or so may be treated more fully than the rest. But when one asks why those particular societies have been chosen, the only answer generally seems to be that they are those with which the writer is personally familiar, which have been described by his friends or immediate colleagues, which have come to be regarded as "classical" instances, or on which a new book or article has recently appeared. Often enough, too, language seems to have been a determinant; all the authors mentioned above, with the notable exception of Lowie, refer almost exclusively to the literature in English. Certainly, on the whole, there is little evidence in such chapters

of any attempt to attain systematic and thorough coverage. They may be considered comparative only in so far as they use illustrative material from a variety of different peoples. That they are often stimulating and fruitful in hypothesis is a tribute more to the author's ingenuity than to his method; invariably there are too many gaps in his material for us to be convinced that his findings are conclusive.

Another type of approach is found in *African Systems of Kinship and Marriage*, edited by Radcliffe-Brown and Daryll Forde. As the title indicates, this is a regional study. It consists of a short preface by the two editors, a lengthy introduction by Radcliffe-Brown, and nine essays by different contributors, each discussing one or more African peoples among whom he has done fieldwork. The editors state that "The purpose of the volume is to present a general view of the nature and implications of kinship in Africa," and claim that "save for the peoples of the smallest scale of social organization, the Bushmen and Negritos, and the kinship pattern among the Amhara of the Ethiopian Kingdom, for none of which studies were available, the chief varieties of kinship organization occurring in trans-Saharan Africa are illustrated and considered." (p. vi)

If this claim refers to the individual contributions, it is invalid: Radcliffe-Brown in his introduction discusses at some length several African peoples (such as the Masai, Nandi, Shona, and Nkundo) that are not described in the body of the book. The mere fact that it has been found necessary to refer to them in this way shows that the essays do not, in fact, illustrate "the chief varieties" of kinship organization in Africa. This conclusion is strengthened if we look at the list of contributors. Without exception, they are British or South African anthropologists who received all or much of their training at London, Oxford, or Cambridge. No foreign anthropologists are represented, and all the territories covered form part of the British Commonwealth in Africa. In brief, the choice of contributors, and consequently of the societies discussed, seems to have been determined more by personal predilections than by a careful plan to provide systematic and adequate coverage of kinship in Africa generally.

The last type of comparative method that I wish to discuss at this stage is illustrated by Murdock's *Social Structure*. In this book, he discusses a " single aspect of the social life of man—his family and kinship organization and their relation to the regulation of sex and marriage." (1949: vii). The study is based upon data provided by 250 different "societies," which Murdock regards as the number of cases "required for reliable statistical treatment (1949: viii) and he makes great use of statistics for establishing correlations of various kinds. Ideally, as he indicates elsewhere, the societies selected for comparison should constitute "a statistically representative sample of all known cultures, primitive, historical, and contemporary" (Murdock and Ford:

xii); and in a more recent paper, dealing with the stability of marriage in non-European cultures, he does in fact compare forty societies, eight chosen from each of the world's major ethnographic regions (Asia, Africa, Oceania, and native North and South America); "Within each region the samples were carefully selected from widely scattered geographical locations, from different culture areas, and from levels of civilization ranging from the simplest to the most complex. . . . The selection was made in as random a manner as possible except that it was confined to cultures for which the descriptive literature is full and reliable. . . . The method, it is believed, comes as close to that of purely random sampling as is feasible today in comparative social science." (1950: 195)

At first sight, this appears to be a highly commendable approach: if we cannot deal with every known society, a sampling technique of the kind described does at least suggest that adequate consideration has been given to the problem of eliminating bias from the selection of the societies that are to provide the raw material for comparison. But in *Social Structure* Murdock was unable to adhere strictly to his own rules of method. "The present author," he says, "has departed from plan mainly in abandoning the sampling technique in favor of using all available cases in areas such as South America and Eurasia for which there are too few sufficiently documented cases to obtain an adequate sample. In other areas, too, he has occasionally chosen a society because a good source was readily accessible rather than because a sample was demanded. He has, however, sought consciously to avoid any appreciable overrepresentation of particular culture areas. In short, departures from strict sampling, where they occur, reflect availability or non-availability of suitable sources and no other basis of selection." (1949: viiif).

To this explanation no exception could be taken, if it did in fact account adequately for Murdock's "departure from plan." But our faith in its accuracy is shaken if we examine closely the nature of the sources that he has used. Of the 315 ethnographic works (excluding personal communications) listed in his bibliography, no fewer than 302 (96 per cent) are in English. This, surely, can hardly be ascribed to "non-availability of suitable sources" in other languages? The ethnographical literature on Africa, for instance, includes many works in French, German, Flemish, etc., which contain the kind of data sought by Murdock; but for 62 of the 65 societies selected from that continent he relies on English sources alone, and for the three others he uses respectively one English and one German source, two English and one German, and one English and one French. And although he includes several Indonesian societies in his sample, he again relies solely on English sources, and does not once refer to the voluminous literature on that region in Dutch.

Moreover, as he himself states, for 165 of his selected societies he "had to content himself in most instances with a single book or article," in contrast

to "the complete source coverage" for the 85 other societies, about which data were extracted from the files of the Yale Cross-Cultural Survey (1949: viii). It is perhaps not surprising, therefore, that he sometimes goes wrong in attributing a particular society to one or other of the "eleven major types of social structure" which he defines; thus, he classes the Pedi of South Africa as "Normal Dakota," whereas, like the neighbouring Tswana, they are really "Normal Yuman," and he classes the Xhosa as "Normal Guinea," whereas, like the Swazi and Zulu, they are "Normal Dakota" (1949: 235, 237). But one hardly expects to be told, in a book of this nature, that "The informed reader who detects factual gaps or errors in our tabulated data will usually find, by referring to the bibliography, that they are due to the failure to use some recognized source. The author's only excuse for his incomplete coverage in the additional 165 societies is that he simply could not afford the extra years of research labor that would have been required to attain the degree of thoroughness achieved by the Cross-Cultural Survey" (1949: viii). We cannot but conclude that Murdock's sampling technique was influenced more by literary convenience than by adherence to scientific discipline, and when he asks us, in effect, to condone incomplete research because he could not spare the time to do his job thoroughly we may perhaps be forgiven for being sceptical about the validity of his correlations and conclusions.

Murdock's statistical approach provides a convenient point of transition to the second topic that I wish to discuss: What is meant by "a society" in comparative work of the kind that we have been considering? The problem was posed many years ago by Hobhouse and his collaborators in *The Material Culture and Social Institutions of the Simpler Peoples*, where statistical method is also employed. "Every rigid statistical inquiry," they say, "supposes that the phenomena with which it deals can be stated in terms of some unit which is constant throughout its field." And they go on to ask, "What is the unit social group?" (1913: 8)

Murdock, too, apparently believes that the units chosen for comparison should have the same characteristics. "Comparative studies of community organization", he says, "are not vitiated by differences in political complexity. One may, however, seriously question the validity of those comparative studies of government which deal with the largest political aggregates in diverse societies, whether they be communities, organized tribes, or complex states. The Arunta band and the Inca empire, for example, are not comparable units, although it might well be profitable to compare the former with the local Peruvian *ayllu*, or the governmental institutions of the Incas with the Dahomean monarchy." (1949: 86)

The problem of "units" has also been raised, in somewhat different form, by Radcliffe-Brown, who writes: "At the present moment of history, the net-

work of social relations spreads over the whole world, without any absolute solution of continuity anywhere. This gives rise to a difficulty which I do not think that sociologists have really faced, the difficulty of defining what is meant by the term 'a society'. They do commonly talk of societies as if they were distinguishable, discrete entities, as, for example, when we are told that society is an organism. Is the British Empire a society, or a collection of societies? Is a Chinese village a society, or is it merely a fragment of the Republic of China? If we say that our subject is the study and comparison of human societies, we ought to be able to say what are the unit entities with which we are concerned." (1940: 4f)

Radcliffe-Brown's own attempt to solve the "difficulty" to which he refers strikes me as inadequate. "If," he says, "we take any convenient locality of a suitable size, we can study the structural system as it appears in and from that region, i.e., the network of relations connecting the inhabitants amongst themselves and with the people of other regions. We can thus observe, describe, and compare the systems of social structure of as many localities as we wish." And he then cites with approval, as examples of "societies" in this sense, a Japanese village and a French Canadian village, both of which were studied by former pupils of his (Embree 1939; Miner 1939). If by this he means that the village is to be the only "unit entity" for purposes of comparison, and that Japan, for instance, is to be regarded as a collection of many thousands of separate societies, then the task of the comparative sociologist becomes virtually impossible. If, on the other hand, he means merely that one can isolate villages for purposes of study, and compare the village systems of different societies, just as one can compare the family or clan systems of different societies, then he avoids the issue that he raised.

Murdock, without defining his terms, equates "societies" with "individual cultures" or "individual tribes," which he treats as "independent units for statistical purposes" (1949: x). In a more recent publication, he defines a culture "as including all local culture variants exhibited by communities within a particular geographical area which speak mutually intelligible languages and have essentially similar forms of economic adjustment", and speaks of the "tribe" as the "social correlate" of a culture (1953: 478f). If this is the sense in which he uses the terms in *Social Structure*, then some of his 250 sample societies are in fact not "independent units for statistical purposes"; thus, Pedi and Tswana are "mutually intelligible languages," as are Xhosa, Zulu, and Swazi, on the one hand, and Ganda and Soga, on the other, which means that we have here three "cultures" or "tribes," and not seven as maintained by him.

Hobhouse and his collaborators point out that "the reports of ethnographers sometimes deal with tribes, sometimes with divisions or branches of a tribe, and sometimes with groups of two or three, a dozen, or even a score

of tribes taken together" (p. 8), and that the word "tribe" itself is used in many different senses. In the circumstances, they conclude, the only safe procedure is to regard as a society "each group which we find so treated by our authorities" (p. 9). This, in effect, is what Murdock also does, as do most of the other writers to whose treatises I have referred above.

Nadel has recently suggested that "society" can best be identified with the "political group," i.e. with "the aggregate of human beings who coordinate their efforts for the employment of force against others and for the elimination of force between them, and who usually count as their principal state the possession and utilization of a territory" (1951: 187). Hobhouse and his collaborators also discussed the possibility of using political independence as a criterion, but, in my opinion rightly, rejected it as unsuitable, at least for statistical purposes (p. 9). Apart from the fact that it is not always possible to isolate political aggregates (e.g., among the Tallensi described by Fortes), their adoption as units would multiply very considerably the total number of separate societies known to anthropologists, without adding anything worth while. Very many politically separate communities have virtually the same social structure, e.g., the fifty or so "tribes" into which the Tswana-speaking peoples of South Africa are organized, or the small hunting bands of such peoples as the Bushmen and the Andaman Islanders; and to treat each of them as a separate unit would not solve our difficulty, especially as the number of such communities is sometimes almost impossible to ascertain. It is for this reason, too, that I cannot believe Radcliffe-Brown would seriously have us regard the village as the only "unit entity" for purposes of comparison.

However, the solution may be found in yet another kind of comparative approach, the only one that I consider at all helpful in work such as I have been discussing. It consists initially in making an intensive study of a given region, embracing all the peoples living there about whom information is available. By carefully comparing the forms taken among those peoples by the particular social phenomena with which we are concerned—as, in this instance, kinship and marriage—we try to establish, by a process of generalization, one or more basic types into which the various forms can be classified. Thus, the many hundreds of politically separate Bantu-speaking "tribes" in South Africa can be divided, on the basis of marriage regulations, into three major groups, characterized respectively by comprehensive exogamy (prohibition of marriage between all kinds of kin), preferential matrilateral cross-cousin marriage, and marriage permissible with any kind of first cousin.

Such an approach, in my opinion, will enable us in the first place to ensure that all the known "societies" of people inhabiting a given region are discussed, and will therefore do away with the need for sampling or any other arbitrary form of selection. This will meet some of the criticisms that I have made against *African Systems of Kinship and Marriage* and *Social Structure*. Then,

by establishing "types" or "species" of kinship system, government, age organization, or whatever else may be our topic, we shall obtain units of the kind that could be used in wider comparative studies of a continental or even universal scope: instead of comparing individual "societies," we could compare larger groups each characterized by a uniform type of kinship, etc. The units would be not only far fewer than the total number of "societies," and therefore easier to handle, but would also be constant in character, and therefore strictly comparable. This should meet the difficulty of defining "unit entities" for purposes of comparison. And if, as our third step, we proceed to compare the types of kinship system, etc., in adjoining instead of widely separated regions, it is possible that the number of types may be still further reduced, if one or more are found to exist also outside the region from which we started.

Comparative work of this kind will obviously have to be done by many different students, each dealing with an area or group of peoples on which he has specialized. But social anthropology would benefit considerably, and have more right to claim that its methods are adequate, if in the near future far more attention were devoted to intensive regional comparisons. Much has already been accomplished for America and Australia, and there is now enough material available to attempt it for many parts of Africa and the Pacific. Until we have defined basic types of kinship, etc., for most if not all of the world, comprehensive surveys of the kind made in the textbooks, or by Murdock and his school, must continue to be defective, even although often stimulating as leads to further research.

I need hardly add that regional studies of the kind advocated here are not the only form in which comparisons can or should be made. Method must be determined largely by problem, and where the problem is relatively limited in scope the data required may also be limited. Thus, if one is interested in the social composition of the band among hunting peoples, or in the nature of unilineal kinship groups, or in the structural implications of matrilateral cross-cousin marriage, then only peoples with the required characteristics need be considered—provided, once more, that the coverage is comprehensive. But if we aim at describing, say, family and kinship systems of all known human cultures, or even of Africa only, then no other approach is as likely as the regional to ensure that all possible types are included. Murdock, for instance, classifies kinship terminologies into six major types, using as his criterion the terminology for first cousins (1949: 223f); had he used all the available sources for the Bantu of Southern Africa, he would have had to add at least a seventh type, in which agnatic cousins are classed with siblings, cross-cousins are classed together by a common term, and there is also a distinct and separate term for materterine cousins (mother's sister's children).

What I have said above is of course by no means new. Radcliffe-Brown himself, following Steinmetz and others, advocated this kind of comparative work more than twenty years ago, and actually employed it in a classic study of Australian kinship systems (1931 a; b). It has also been used, still in the field of kinship, by Gifford for California, Eggan for the Western Pueblo, Spier for North America generally, Kirchhoff for South America, Audrey Richards for Central Africa, etc. Similar studies, in the field of government, have been made by Lowie for America (1948b), by myself for the Tswana-speaking peoples of South Africa, and by Evans-Pritchard for the Nandi-speaking peoples of East Africa; and I would like to end with one more quotation, this time from Fortes and Evans-Pritchard, who in 1940 edited a volume of studies on African political systems by different writers, similar in nature to the volume on kinship already mentioned and, incidentally, suffering from some of the same defects. "Most of the forms described," say the editors (1940: 13), "are variants of a pattern of political organization found among contiguous or neighbouring societies. . . . Each essay furnishes a useful standard by which the political systems of other peoples in the same area may be classified. No such classification is attempted in this book, but we recognize that a satisfactory comparative study of African political systems can only be undertaken after a classification of the kind has been made. It would then be possible to study a whole range of adjacent societies in the light of the Ngwato system, the Tale system, the Ankole system, the Bemba system, and so on, and, by analysis, to state the chief characters of series of political systems found in large areas. An analysis of the results obtained by these comparative studies in fields where a whole range of societies display many similar characteristics in their political systems would be more likely to lead to valid scientific generalizations than comparison between particular societies belonging to different areas and political types."

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SUMMARY OF COMMENTS AND DISCUSSION

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SCHAPERA'S original paper, which was circulated before the conference, was entitled "Some Thoughts on the Comparative Method in Social Anthropology." I do not know the meaning of the change from "thoughts" to "comments," but dropping the "the" from the title of the revised paper is perhaps the most eloquent, if inconspicuous, testimony of the impact of the conference: for Schapera and the other participants agreed that there is no single method of comparison in anthropology, that method is largely determined by problem, and that a method appropriate for the comparison of kinship systems is not necessarily most appropriate for other types of cross-cultural comparison.

This general agreement was not evident at the beginning of the conference but was the outcome of an intense discussion of the questions raised in Schapera's paper. Since it would be impossible to reproduce the details of the dis-

cussion, I shall attempt only to describe the most important issues which were isolated and the extent of agreement or disagreement that was registered in the group.

In its original version Schapera's argument was directed against large-scale comparative studies in social anthropology, particularly in the study of kinship systems. These, he argued, undertake comprehensive comparisons of societies and cultures without regard for the scientific principles to which such comparisons ought to conform: unbiased selection of the samples and clear and consistent definition of the units being compared. There is a method of comparison, according to Schapera, which avoids the difficult problem of sampling and definition of stable comparable units. This is the method of intensive regional comparison, i.e., "making an intensive study of a given region, embracing all the peoples living there, and trying to establish one or more basic types into which the social institutions of those peoples can be classified."

This method, Schapera believes, insures that all known groups of people in a region are covered. It also leads to the classification of those groups into "social types," which, unlike "societies" or "cultures," provide a stable unit that can eventually be used for wider comparative studies of continental or universal scope. But these wider comparative studies cannot be adequately carried out, Schapera thinks, unless the intensive regional studies are done first. He therefore proposes that anthropologists give research priority to the intensive studies.

It was fully acknowledged in the discussions that in recent comparative studies the selection of cases was not rigorously unbiased and that the units compared were not always comparable. But Schapera's specific criticisms and the conclusions which he drew from them were not accepted. It was felt that many of the difficulties he raised about the limited number of cases, and the reliance on a small number of authors and languages, the shifting size and definition of units, etc., were not too serious if the quality of the data is good and the context of the problem is taken into account.

Stout, e.g., pointed out that Murdock did try to apportion his sample according to social types and cultural patterns, and that in his context of comparison, differences in size are not an important obstacle. Others felt that Schapera was too pessimistic in his conclusions about finding suitable definitions of "societies" and "cultures" as units; they suggested that adequate definitions might be found by comparing "total social systems" (Aberle), common language and cultural groupings (Bascom, Stout), units of equal size (Baerreis), etc. For the classification of African tribes Bascom proposed that the common tribal names assigned to a given tribe by its neighbors might prove useful as starting points.

Comparisons based on a few geographically scattered cases were defended

against Schapera's strictures, on the grounds that they frequently lead to the development of fruitful hypotheses and typologies. Hart, confuting one British anthropologist with another, cited Fortes' recent *American Anthropologist* article on unilineal descent as an example of such fruitful violation of the intensive method of comparison:

"What he (Fortes) is doing is analysing various African solutions to a number of problems which are not narrowly African problems but universal problems. His paper might be roughly described as 'what a group of widely scattered and arbitrarily selected tribes do about such things as descent, balancing lineage affiliation with tribal affiliation, etc.' "

Redfield was of the opinion that it may prove more effective to compare widely separated societies for their contrast value:

"The effectiveness of Benedict's *Patterns of Culture* is partly a result of a choice of societies, from separated parts of the world, that emphasize a range of type, considering type now as a conception with qualities suggested by individual psychology. Malinowski compared the Trobriand society with Western European society, only with regard to evidences for the Oedipus complex: for the question he asked at the stage of enquiry then reached, the choice of entities to be compared was a good choice. Indeed, no comparison of societies at all is needed in the creation of hypotheses: one may derive tentative generalizations from study of a single entity; it is often done."

Steward also supported the utility of analyzing a single case as well as the comparison of widely scattered ones for the purpose of deriving "cross-culturally significant generalizations about form, function, and process."

It readily became apparent in this phase of the discussion that the problems of comparative method could not be discussed apart from the aims and problems of comparison. Where the aim is to give a comprehensive survey of the kinship systems of all parts of the world or of a large area, a method like that of intensive regional comparison is probably most appropriate. But if the aim is to derive and test cross-cultural generalizations, or to compare total cultures and "national characters," or to ascertain how different cultures have solved certain universal problems, then other and perhaps novel methods and concepts will be required.

Even for those problems for which the intensive method of comparison is appropriate, there was considerable scepticism concerning Schapera's claim that it is a method that avoids the difficulties of sampling and of identifying comparable units. Bascom pointed out that not only regional studies like *African Systems of Kinship and Marriage*—whose weaknesses Schapera admitted—but those like *African Political Systems*—which Schapera takes as a model of the intensive method—fail to provide the systematic coverage claimed for them. The latter, Bascom says, is arbitrarily restricted to British Africa,

"with the great areas of French and Portuguese Africa and the Congo omitted. It is difficult to see, for example, how African kinship can be discussed without taking into account the *oruzo* and *eeanda* of the Herero, or how African political systems can be discussed without some consideration of the great empires of the Western Sudan or the kingdoms lying along the Guinea Coast. Schapera's final quotation is taken from a section in *African Political Systems* entitled 'A Representative Sample of African Societies,' but in neither book is there evidence of systematic sampling or of any justification for the title."

The ambiguities of "unit" and "type" in regional comparison are revealed in their most general form in the following questions posed by Redfield:

"In coming to know the regional type, how does one select among the entities available within that region for study? We are to make an 'intensive study of a given region, embracing all the peoples living there.' Does this mean we are to make an intensive study of every human being living there? If not, what is to be the basis of selection from among them? If we are to study intensively all the component sub-entities within the region, what are these entities? He quotes R-B to the effect that Australian tribes are to be chosen. Yet Schapera has already pointed out the difficulty of defining tribes, or societies of any sort. Further: What is to guide us in determining the limits of the region for which the constructed type is to stand? We are told (as R-B describes "the method") that there are at least two "units" among Bantu cultures. What is to guide us to recognizing just these units? And still further: If the types constructed for the political systems embrace some peoples or parts of regions not embraced by the types of kinship systems (the problem of overlapping types), does this mean that our construction of types is in error, or does it mean that the region for which the type stands varies with the type?"

The discussion revealed far less confidence than Schapera had in the clarity and stability of regional "social types" as units of comparison. Many distinctions and refinements were introduced in order to clarify this concept. Hart said that

"Radcliffe-Brown has always used the phrase in two quite different senses. He sometimes uses it as meaning a group of societies territorially defined—Australian societies, Bantu societies, etc. At other times he uses it with a structural definition—'tribes with clans', tribes with organized chieftainship, tribes with a Choctaw type kinship system. I would like to see Schapera take up and make explicit this second sense. If we stick to the first sense—that of groups of tribes locally defined—we can only remain regional specialists (as in fact Evans-Pritchard has advocated), and only explain phenomena therefore in terms of particularistic conditions prevailing in the local regions."

Bidney distinguished generic and specific types; Eggan doubted that it is possible to identify the same "social types" in different cultures; Tax suggested that this difficulty arises from our ignorance of the causes of similarities and differences; and White attributed it to our timidity in making generalizations.

Bascom, Redfield and others wanted to know what a "social type" would refer to when the political, kinship, religious, economic "types" in a region do not coincide.

Steward introduced a measure of harmony into the discussion of "types" when he proposed to distinguish between "similarities" within a culturally homogeneous area and "regularities" of structure or process which might recur in different cultures and areas. Regional comparison leads to the discovery of similarities, cross-cultural comparison to the discovery of regularities. It was agreed that both kinds of comparison are needed and may be pursued simultaneously with mutual benefit.

I shall not try to summarize Schapera's rebuttal to these criticisms since the revision of his paper itself constitutes the rebuttal.

The Lebanese Village: An Introduction*

By JOHN GULICK

I

LEBANON is a rugged, mountainous, and generally well-watered country. Since many of its localities are ideally suited for defense, it has long served as a refuge area for religious minority groups which have an agricultural economy. Until very recently, communications were poor, permitting the various sects and their individual villages to remain isolated, independent, and generally hostile.

Because of the scarcity of arable land, and in the best interests of defense, the typical village is a tight cluster of houses, with few, if any, isolated farmsteads. As a physical entity it is clearly distinct from its nearest neighbors. The surrounding fields are generally divided into small, individually owned plots, supported on tiers of stone terraces whose construction and maintenance require prodigious labor. For the most part, the farmer raises orchard and vineyard crops for his own consumption and for sale, while a large proportion of the wheat which he needs for food must be imported from the plains which lie to the north and east of Mount Lebanon. The results of his labors are an adequate, though frugal, diet raised, for the most part, on ancestral land. He has, also, an abiding sense of having added to the already abundant natural beauties of the country with which he strongly identifies himself.

II

A clear picture of the general characteristics of the culture of the Lebanese village may be found in one work of Afif Tannous (1944). Although he writes of the Middle East as a whole, what he says is definitely applicable to Lebanon. It is a picture of a discrete, compact social unit whose inhabitants constitute a strong in-group vis-à-vis all outsiders. It is not surprising to learn that village endogamy is the rule. Life within the village is dominated by three foci of interest whose influences on behavior tend to be interwoven with each other. Stated in a most abbreviated fashion these are: Kinship, Religion, and Devotion to the Land. At this stage of investigation, it is the present writer's impression that kinship is the most important of the three, but this is by no means a final conclusion. It is, however, the reason why kinship is given particular attention in this paper.

The kinship structure of the Lebanese village is based upon the segmenta-

* The conclusions expressed in this paper are drawn, in part, from the findings of a community study which was conducted by the writer in Lebanon, January through July 1952.

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tion of the population into patrilineages. (Tannous refers to these as "kin groups" in the paper cited above.) These patrilineages are not merely genealogical constructs. They are very active social groups. Lineage endogamy frequently occurs, especially when the lineages involved are large. Islam actually encourages lineage endogamy, indeed, even extended family endogamy. It also tends to be encouraged by the Christian Arabs who usually forbid the marriage only of first or second cousins, depending on the sect. The lineage often holds communal land and often inhabits its own special section of the village, cooperation among its members is expected, and it is the basic loyalty group called into play in the family feuds famous in the region (Tannous 1942b: 236).

The genealogical relationship among the lineages varies from village to village. Except where lineage endogamy prevails, they are united by a complex network of marriage ties, and it should be noted that, at least as far as sentimental attachments are concerned, close matrilateral kin as such are by no means ignored by the individual. In regard to ultimate connections, two types of arrangement are known to exist. In the village of Bishmizzeen, in northern Lebanon, the lineages are of separate origin, each being descended from men who came to the site independently of each other (Tannous 1942a: 62–63). Identical conditions obtain at Ras el-Metn in central Lebanon (Févret: 273–274). On the other hand, in the village studied by the writer, which is much smaller than the other two, everyone can trace his descent from one of three brothers whose father is recognized as the founder of the present community. He lived about twelve generations ago, and, in the last analysis, all his descendants constitute a single lineage. As a matter of fact, the villagers consciously regard themselves as all belonging to one big family. However, they are segmented into twenty-one patrilineal groups, averaging six generations in depth, each of which has the characteristics of a full-fledged lineage. The writer would guess that the situation in Bishmizzeen and Ras el-Metn is the more common, but this cannot be proved. Actually, it does not make any fundamental difference from the point of view of social behavior. The second situation, universal descent from a common ancestor, only intensifies the already extreme sentiment of village unity which is characteristic of Lebanese villages as a whole. This sentiment, it is important to note, counterbalances, and in fact, ordinarily outweighs whatever internal divisions centered around lineage loyalties occur in the village.

III

The system of kinship terms in use in the village studied by the writer is identical in every important respect with the Palestinian Christian system reported by Davies (pp. 244–246). The system is bifurcate collateral in type, and so it makes possible the specific designation of all types of first cousin, male

and female. Of these, only patrilateral parallel cousins are necessarily members of ego's lineage, with all the rights (including, theoretically, the inheritance of property) and obligations which this involves. It has already been implied that Islam favors the marriage of patrilateral parallel cousins but the sects to which the Christian Arabs belong forbid marriage between *all* types of first cousin. First cousin marriages do sometimes occur among the Christians but they require an episcopal dispensation. There are two such unions in the village studied by the writer, and both are looked upon by the other villagers as being exceptional, though not in any way reprehensible. Both are cases of the marriage of matrilateral first cousins. It was stated that the bishop would probably refuse to grant a dispensation in a case of patrilateral cousins who wished to be married.

Ego's patrilateral first cousins are members of his lineage, but in general, lineage affiliation gives direction to behavior in special or crisis situations, and not so much in day-to-day living. In everyday matters ego tends to regard all his first cousins as being equally close relatives. This indicates an apparent conflict with two firmly established patterns in Arab culture: the dominance of patriliny and the preferential marriage of the closest possible relatives outside the nuclear family. As far as the writer is aware, the problem only involves the Christian Arabs, and he believes that it reflects a sort of compromise growing out of the cultural conflicts which arose when the pagan Arabs were Christianized. This is a very interesting and complex topic, but it cannot be pursued any further in the present paper. The favored status given to matrilateral first cousins is not extended to any more distant matrilateral relatives. From the point of view of differentiation, the correspondence between cousin terminology and cousin behavior is, all things considered, fairly high, although the terminological distinction between cross and parallel matrilateral first cousins does not appear to match any patterned distinctions between them in attitude or behavior.

The intensive research which was undertaken by the writer yielded information which makes possible certain refinements and extensions of a number of Davies' definitions of Arabic kinship terms. Of these, two are especially germane to the main subject of this paper. In a general sense, Davies is correct in saying (p. 246) that the term *ṣihr* designates a male affinal of ego who is of ego's generation or younger, but this statement fails to bring out the full significance of the term. *Ṣihr* is applied to a man of ego's generation or younger who marries any woman belonging to ego's lineage and also (in the present writer's village, in any case) to a man who marries ego's matrilateral first cousin. In addition, the term is used by any member of the village to designate an outsider who marries any woman belonging to the village. This last usage reveals the way in which village unity is conceived, or at least verbalized, in kinship terms.

The term *'amm* designates father's brother, and it is also used (in address only) for ego's spouse's father. Davies briefly discusses (p. 250) the long-accepted belief that this extension in usage reflects the relationship which is implicit in the Islamic (and probably pre-Islamic) ideal of patrilateral parallel cousin marriage, namely the identity of father's brother and father-in-law. But why should this usage have persisted for many centuries among the Christian Arabs? The writer believes that the most satisfactory explanation is as follows: *'amm* is regularly used as a respectful form of address by any villager to any other male villager of his parents' age, unless the older man is a mother's brother (*xaal*). Ego always addressed his (or her) father-in-law-to-be in this manner, and the practice is simply continued after marriage. There is a special term for father-in-law, but it is used referentially only. Village informants stated emphatically that an older person *must* be called *'amm* (or *'ammit*, if it is a woman). To use the personal name alone would be disrespectful, and so, in a different but very significant way, would be the use of the Arabic equivalents of "Mr." or "Mrs." These titles are addressed only to complete strangers (i.e., outsiders). Consequently, their application to village-fellows, whatever the nature of the personal relationship might be, is simply inconceivable. Here, then, is another symbolization, in terms of kinship, of certain general status relationships, and of the village in-group.

IV

The essential nature of this strongly unified village, viewed in terms of kinship structure and marriage rules, can be brought into relief by comparing it with and contrasting it to several of the types of social units which have been defined and analyzed by G. P. Murdock.

As far as its local endogamy and strong in-group orientations are concerned, the Lebanese village resembles the *deme*, but the *deme*, by definition, is not segmented by unilinear groups (Murdock: 62). On the other hand, while kinship plays an essential part in the patterning of social relations in the village, it is not, for two reasons, a *clan-community* (Murdock: 74). In the first place, its members do not necessarily constitute a single lineal unit. In the second place, Murdock's clan-community is exogamous.

Knowing that intra-village lineages are often exogamous, despite preferential lineage endogamy, and knowing that lineages are generally localized within the village, we are strongly tempted to say that the Lebanese village is a system of *clan-barrios* (Murdock: 74), and that as such it could be classified as a *segmented community* (Murdock: 89). It is certainly a community, and a segmented one, but there are four reasons why it would be an error to apply the term, on Murdock's definition of it, to the Lebanese village. First, the intra-village localization of lineages, though a real tendency, does not result in the very clearly defined "village-within-a-village" type of unit which is

characteristic of the barrios in, for example, Tepoztlán, Mexico (Redfield: 69ff), or, for that matter, of the *ḥara* in the traditional structure of the Middle Eastern city (Gibb and Bowen: 277–279). Second, the available data do not allow us to conclude that those lineages which are clearly localized are also exogamous. Third, such a classification would not take into account the fact that endogamous lineages, also, frequently occur, and are, in general, preferred. Fourth, and most important, the classification would fail explicitly to emphasize the firm unity and sharp identity of the village as a whole.

However, there can be no question but that the Lebanese village is an excellent example of the operation of the emphatic in-group attitude which Murdock discusses (p. 83) as being an important factor in community structure in general. The writer intends, in fact, to pursue this subject at greater length in a future publication. In the meantime, the Lebanese village as a type of kinship structure may be defined as *an endogamous local group which is segmented into patrilineages which are preferably endogamous but often exogamous in practice.* Residence after marriage is ideally patrilocal but often neolocal in practice.

Kinship and marriage patterns thus are an integral part of the extreme localization of interests and activities which is the hallmark of village culture. The same thing is revealed in the other two major foci of interest which Tannous has identified. Religion provides the rationale for rites of passage and intensification which generally play an important part in periodically reinforcing feelings of village unity. In addition, religious affiliation is the major criterion by which the nature of extra-village relations is defined. The barriers of suspicion and hostility which generally divide one village from another tend to be minimized if the same religion is shared by them, intensified if it is not. Under Devotion to the Land may be subsumed a number of values and emotions. Among the most important of these, perhaps, is the association of farming with land use and inheritance patterns both of which are determined by kinship organization. Also, farming is the set of acts which gives reality to the powerful feeling of identification with nature to which reference has already been made.

This discussion cannot be closed without pointing out that the kinship system of the Lebanese village has persisted for many centuries, and that there is some evidence which indicates that it has withstood remarkably well the disintegrative influences of recent intensive culture change. In the village studied by the writer, the third of Tannous' foci of interest, Devotion to the Land, has been dealt a severe, if not mortal, blow. The analysis of the reasons for this lies beyond the scope of the present paper. It is enough to say here that only a few of the older men are still full-time farmers. A significant proportion of the younger men earn their living by holding a variety of urban jobs, and they live away from the village most of the time. Education, which is of major importance in the villagers' scale of values, is definitely leading the

younger people away from the village in terms of ambitions and attitudes toward work. There are, furthermore, indications that religion as a system of theological faith has lost much of its vitality. But beyond this, the process of "disintegration" has, for the moment in any case, come to an abrupt halt. Rites of passage and intensification continue to function effectively in cementing village unity. Those who live away feel compelled to return to the village at such times as Easter and when funerals take place. The kinship structure, with its associated attitudes, stands almost unshaken. The chief threat to its integrity—marriage to outsiders—is a new trend which appears to be increasing in frequency, but the number of people who have married outsiders is relatively small considering the extensive opportunities which most of the villagers now have for interacting with outsiders, and marriage to outsiders does not appear to have had any influence on the feelings of village unity and loyalty which are entertained by the people as a whole.

V

Such is the nature of the cultural milieu of about 800,000 rural Lebanese. The writer is of the opinion that the Lebanese type of village organization is, with modifications, widely distributed throughout the Middle East. Or, to put the matter in its proper perspective, the Lebanese village is believed to be a regional variant of a type of village characteristic of the Middle East as a whole which features unilinear kinship organization, kin-group and local group endogamy, and in-group fixation.

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Age-grading in Japan[1]

By EDWARD NORBECK

THE emphasis upon age in Japanese culture has long been noted by Western observers. Numerous references to this emphasis may be found in both implicit and explicit form in Western accounts of historical and modern Japan. The subject of emphasis upon age as manifested in age-grading and age-classes, although virtually absent from Western publications, has in recent years received attention from Japanese social scientists. Although no comprehensive study has as yet emerged in Japanese, present accumulations of data appear to indicate that practices of age-grading and the existence of certain formalized, named age-classes were common if not general among the peasantry of Japan in the past. An attempt will be made here to review and appraise the data on this subject presented in such works by Japanese scholars as are at hand. To the knowledge of the author only one American publication (Matsumoto 1949) deals with age-grading in Japan—and only peripherally. T. Omachi and K. Segawa, who are among the Japanese social scientists interested in this topic, both state that their research in this field has just begun (personal communications). Data are also drawn in part from the author's field work in Japan.

Practices of age-grading which appear to be very ancient still exist in a number of rural communities of Japan. Of the formalized age-classes which once existed, however, only those composed of young men and young women are common today. Present-day communities which retain more or less clearly defined age-classes applying to all persons are uncommon, and such communities are usually isolated geographically from urban centers. Only vestiges of age-grading practices remain in the more modernized and urban areas, with the exception that age-classes for young, unmarried persons exist in communities which can be called neither backward nor isolated. Clearly defined age-class systems covering the whole life span of individuals appear to have become relatively rare by the end of the nineteenth century even in rural Japan, where ancient customs have lingered the longest. There is, however, a quantity of evidence of their former widespread existence. Material at hand on past or present practices of age-grading covers a wide area of Japan, all of the major islands except Hokkaido (on which the author has no data whatever), as well as the Ryukyu Islands and the Island of Tsushima in the Japan Sea. A description of age-grading customs as they appear to have existed in much of Japan until a period perhaps less than a century ago will be attempted here. In some instances, as may be noted, the practices continued until a much later date or exist today.

INFANCY AND CHILDHOOD

A few instances of the present existence of associations composed of pre-adolescent children (*kodomo-gumi*) are cited by Japanese scholars. These associations are reported as active during religious festivals, when they are charged with the performance of simple duties on the behalf of the community. Such children's associations, some of them described as strictly unified groups under the leadership of older children, may also have regularly assigned secular duties such as driving insects from rice fields (Ōmachi 1942: 16–17). "Traces" of children's associations are also reported from a number of communities (Sakurada 1942: 90). Exempletive of such traces is the community of Takashima in the Inland Sea (Okayama Prefecture), studied by the author. Although no children's association has existed within the memory of presently living persons, pre-adolescent children (at present, elementary school children, of ages six to about fourteen) still perform traditional duties, the origins of which are lost in antiquity. Large quantities of firewood are prepared solely by this group of children, under the direction of the eldest and strongest members, for two annual all-night shrine watches in which both children and adults participate. This is a task which normally requires the free time of the children for several days.

The importance of children's age-classes in former days is said to be indicated by the existence of a number of customs surrounding the attainment to certain years of life during childhood (Ōmachi 1942: 16–17). It is possible that in former times more than one age-class of pre-adolescent children existed in some communities. One class was possibly composed of children up to about the age of seven years. It is also suggested that children were not recognized as members of human society until they reached this age (Ōmachi 1942: 11). A second class was composed of children from about seven to approximately the age of physiological puberty. A number of customs revolving about attainment to the age of seven years were formerly and are still observed over a wide area of Japan. Ōmachi (1942: 10) states, "The age of seven years seems to have . . . been considered to be the transition stage from infancy or early childhood, to boyhood or maidenhood, and all sorts of ceremonies are observed by the parents to have the child pass the stage safely." The ceremonies referred to usually include a visit to the shrine of the tutelary god of the community, and may include purification ceremonies, food restrictions and, for boys, the shaving of the head. These ceremonies appear to be closely associated with ancient Shinto beliefs concerning ritual impurity from contact with blood or death (Norbeck 1952). The observances served originally, at least, to remove lingering uncleanness remaining with a child from the defilement incurred at its birth (impurity from the flowing of blood) or uncleanness from other sources, such as contact with dead bodies of related persons, incurred during the passage of these first seven years of life. Purification ceremonies, as such,

for children at this time of life appear no longer to be observed in most communities although shrine visits and other simple ceremonials still exist.

In a number of communities it was the practice until recent years to bury children under the age of seven in separate graveyards (in some instances, children under five years or six years, and, less commonly, those under three years). It was also at the age of seven or eight years that children were first registered as family members in the records of reconversion from Christianity during the Tokugawa period (1615–1868). (Christianity, introduced by the Portuguese in the 16th century, was outlawed in the 17th century. As a measure against Christianity, it was legally required that each person declare, at the time censuses were taken, the religious sect to which he belonged.) "The child whose existence had been recognized by its family, relatives and neighbors by this time became a regular parishioner of the tutelary shrine and a member of the community after passing the stage of the seventh year" (Ōmachi 1942: 10–11). The ceremony called *Shichigosan*, "Seven-five-three," which is observed today throughout much if not most of both rural and urban Japan is perhaps worthy of note here. *Shichigosan* falls on November 15th by Gregorian calendar in the cities, and usually on the 15th day of the eleventh lunar month in rural communities. On this day, seven-, five-, and three-year-old children are taken to shrines, most frequently to the shrine of the local tutelary god. This custom is today interpreted as a ritual act to promote the welfare of the children through supernatural aid.

The feeling of unity among persons of the same age is emphasized in the reports of some Japanese scholars. It is the belief in a number of communities that fortunate or unfortunate occurrences in the life of one individual are prone to affect other persons of the same age or of the same age group. This belief appears to apply especially to children. Thus, on the Island of Takami in the Inland Sea, when a child dies it is the custom to place the lids of cooking pans momentarily over the ears of all children of the same age so that, symbolically, the news of the death may not reach them. Otherwise, children of the same age may also be drawn off to death (Kagawa-ken Minzoku Chōsakai: 29). Similar practices, frequently called *mimi-fusagi* or *mimi-futagi*, "ear-blocking," are reported to exist in a number of communities of rural Japan, especially in Kyushu, and may apply to adults as well as children. Among the various objects held to the ears, rice-cakes, millet cakes, and beans are reported as common. "Ear-blocking" is sometimes also closely associated with beliefs of ritual pollution and serves as a method of avoiding the pollution which may be communicated to all persons of the same age in the community (Ōtō, Makita, Konno). In a number of communities an individual (especially a child or adolescent) does not attend the funerals of persons of the same age for fear of joining them in death, and also because pollution from contact with death is thought to be especially serious for persons of the same age or

of the same age-class. Weddings, wedding feasts and other festivities in honor of an individual are, in some communities, never attended by persons of the same age. Conversely, in other communities, there is a strong feeling of obligation to give aid, or at least be in attendance, upon all occasions of importance in the life of persons of the same age or age-class (Makita: 251–254; Ōmachi 1942: 17). Ideas of affinity by age may even extend to unborn children. The belief is reported in a few communities that if two related women are pregnant at the same time, there is a special affinity between the two foetuses which takes the form of a sort of contest for survival; one will be "conquered" by the other and die (Ōmachi, personal communication).

ADOLESCENCE AND EARLY MATURITY

As previously noted, young men's associations are common in Japan. A distinction must be made here, however, between the modern *Seinendan* and older types of young men's associations which may also have been called *Seinendan* but were known as well by a variety of local names. The latter are often referred to by Japanese social scientists as *wakamono-gumi* or *wakashū-gumi* to distinguish them from local units of the national *Seinendan*. All three of these terms may be translated as "young men's association." The *Seinendan*, although in one sense certainly an example of age-grading, are a modern development brought about under official pressure. *Seinendan* is the term applied to a national organization of young people's associations instituted and bolstered by the government in pre-war years for purposes of promoting nationalism. According to a publication of the Japan Tourist Bureau, there were in the cities, towns and villages of Japan in 1937 a total of 15,469 of these associations for young men with a membership of 2,456,505 persons, and 13,537 similar associations of young women with membership totalling 1,507,778—and also a large number of "junior associations" for younger boys and girls (Yoshida and Kaigo: 86–87). In most rural communities of Japan these organizations still exist, but they no longer receive subsidization from the government and their nationalistic flavor is gone. Membership is composed of unmarried young men and young women from about age sixteen upward. Girls' organizations are frequently called *Joshi Seinendan* and they are closely associated with or merged with the young men's associations. When a boy or girl marries, membership in the *Seinendan* is usually considered terminated. Local *Seinendan* are ordinarily charged with the execution of tasks to promote community welfare and serve to meet community emergencies such as fires or storms. They are also to a large extent social, recreational, and educational organizations.

Traditional young men's associations of a different nature, however, appear long to have been common, and in many instances these older associations appear to have become metamorphosed under government pressure into the modern *Seinendan*. As reported by Nishikiori, for example, a young

men's group called *Waka-zei* existed in Tōgō-mura until the early years of the Meiji period (1868–1912). A young men's association called *Seinendan* was established in this community in 1907, and became part of the national *Seinendan* organization in 1916 (Nishikiori: 77, 90). Some of the modern *Seinendan*, although bearing a superstructure which conforms with the pattern of the national *Seinendan*, still retain much of their old character.

A large number of young men's associations older and earlier than the nationalized *Seinendan*, and which I shall here refer to as *wakamono-gumi*, are reported by Japanese scholars. Recent studies of the characteristics of the *wakamono-gumi* report that admittance into this age-class occurs most commonly either at fourteen or fifteen (Konno 1949: 207) or between fifteen and seventeen (Segawa 1947: 48). *Wakamono-gumi* (and sometimes modern *Seinendan* also) are often subdivided into three groups, bearing such names as *Kowakashū*, *Chūwakashū*, and *Ōwakashū* (Little, Middle and Great Young Men's Associations) (Segawa: 48; Matsumoto: 66). These subdivisions are based upon age and there is local variation in the age limits for each sub-class. Special feelings of attachment are reported to exist between persons initiated in the same year, and instances of close personal friendship akin to "blood brotherhood" may exist between young men of the same ages (Segawa: 46–48; Embree: 87–89). Taboos against attending funerals of persons of the same age may apply throughout the whole life span but they appear to apply particularly to children and young adults (Segawa: 46–48; Ōmachi 1942: 17).

In some communities the idea of affinity between members of the young men's association extends to a feeling of joint responsibility for misbehavior of any member, and erring youths are taken into hand by the other members. There are also often special names such as *aidoshi* or *morodoshi*, "person of the same age" or *tsure*, "companion," used mutually among persons of the same age or who were initiated in the same year (Segawa: 47; Ōmachi 1942: 17). In a number of communities, members of the young men's association sleep in special dormitories (Ōmachi 1942: 12, 14–15; Matsumoto: 66).

Puberty ceremonies are closely associated with initiation rites into *wakamono-gumi* and appear in the past to have ceremonially marked the entrance of a boy or girl into the youths' age-class. On this point, there is danger of assuming that where puberty ceremonies existed, formalized young people's age-classes also existed. This appears not always to have been the case. One finds, for example, in both Japanese and Western accounts frequent reference to a traditional coming of age ceremony known as *gembuku*, "rechristening," which was observed by the royal heir and nobles (for whom there appear to have been no institutionalized age-classes) as well as by common people. For the heir presumptive to the throne, after A.D. 864 *gembuku* consisted of the receiving of a collar. Young men of nobility had their hair arranged in court fashion at this time and received nobles' caps (Papinot: 115). Among common people, at least as early as the Tokugawa period (1615–1868), boys

had their heads partially shaved in a style called *nakazori*, which served as a symbol of attainment to young manhood. Both boys and girls commonly were presented with loin cloths at this time and feasts or parties might be held in their honor. It was also customary to select a godparent for a boy, who might himself do the head-shaving. The year of life when these ceremonies were observed varied locally. In some communities observance was closely tied with physiological puberty of the individual; in others it was at fixed ages varying from about twelve to seventeen. Most of these practices vanished during early Meiji after contact with the West. Coming of age ceremonies are reported still to be observed, however, in some rural communities and may also constitute initiation into the *wakamono-gumi*. Included among the customs still observed are the presentation of loin cloths and the holding of parties or feasts in the honor of a young man or young woman (Ōmachi 1942, 1949; Konno: 208–210). Among variant customs reported are the bringing of a basket of white beach sand to the incumbent head of the *wakamono-gumi* by a young man at New Year's when he becomes sixteen, and the treading down of snow in the path from the community dwellings to the village office and community shrine by new entrants (Sakurada: 88–89). Presentation to the *wakamono-gumi* of a bottle of rice wine by the novice is reported as fairly common (Konno: 208). Ceremonies to celebrate majority (whether or not this at the present time also entails entry into the *wakamono-gumi*) and initiation ceremonies into *wakamono-gumi* frequently involve visits to the shrine of the local tutelary god. Initiations are most frequently held on festival days, especially at New Year's and during the Buddhist *Bon* Festival of late summer (Sakurada: 90; Konno: 207–209).

Wakamono-gumi are everywhere closely connected with the division of community labor. When a young man enters this age-class association, in theory at least, he accepts responsibility for an adult's share of the community work. In communities which engage in joint work to gain a livelihood, such as certain types of net fishing, a boy begins to receive an adult's share of the produce or returns when he is recognized as having come of age. The upper limits of age of young men's associations appear to be quite elastic and in some communities embrace, or formerly embraced, persons as old as forty or more years. The *wakamono-gumi* generally included the most able-bodied persons in the community, males in their youth and in their prime, who were charged with the bulk of the productive labor for the community and who were also the primary group at times of crises such as storms, floods, or fire, which might require strenuous physical effort (Sakurada: 77–96; Konno: 218–223; Ōmachi 1942: 11–12).

Data on formalized age-classes for young women are somewhat scantier than for young men. (The young women of today belong to local *Seinendan*, of course.) The existence of associations composed of unmarried young women (*musume-gumi*) is, however, reported in a number of communi-

ties today and such associations were known to exist until relatively recent years in many communities.

Dormitories for girls of the *musume-gumi* are reported as fairly common in the past, and a few may exist today. Where formalized age-classes and dormitories for young men and young women existed, one of their functions appears to have been to provide an opportunity among the young persons themselves for the selection of spouses. Joint dormitories, for young people of both sexes, appear never to have existed, but nocturnal visits to the girls' buildings appear once to have been customary (Ōmachi 1942: 12–21; Konno: 228–231). The existence of such dormitories and their distribution, together with a number of other Japanese customs, suggests influence from the South Pacific. Dormitories are reported as being more common in southern and central Japan than in northern Japan, and their distribution is more coastal, including the southern coast of the Japan Sea, than inland (Ōmachi, personal communication).[2]

It seems reasonable to state that there is ample evidence of both the past and present existence of institutionalized age-classes for both young men and young women. Many of them are reported to have disappeared during the early years of Meiji (Konno: 205–206). The date of the origin of these age-class associations is unknown, but they are of undoubted antiquity. Matsumoto, in discussing a local young men's association with a known history, states that the oldest extant written "constitution" of a young men's association dates back to 1677, but "The system in Japan, however, probably dates much farther back" (Matsumoto 1949: 65). This seems a reasonable statement, especially since most of the people of Japan, and those of rural Japan in particular, were illiterate until near the end of the nineteenth century. In any case, it appears unlikely that such documents as constitutions for age-classes would have existed in olden times in Japan.

MIDDLE AGE

As noted in the foregoing, the upper limits of age of the young men's associations in some cases included married men of middle age. It appears probable that two overlapping groups based partially on age may have existed in some communities, one composed only of young, unmarried men, and the other of vigorous, able-bodied men whether married or single who were responsible for heavy-duty work. There is little evidence of clearly defined associations limited to married male adults but much evidence indicating the acknowledgement of this group as at least a separate age and status category. Male household heads were formerly and are today the responsible persons in the administration of community affairs. Community meetings, in rural Japan at least, are ordinarily attended by only one member of each household, normally the household head. Household masters are everywhere the leaders of their communities and are everywhere recognized as such. It seems probable

that named, formal associations of mature men of this category were never common.

In this connection, Sakurada's description, as of 1940, of a village age-class system embracing six age categories is pertinent. (It must be noted that the Japanese text does not make it clear whether the classes and assignment of duties as described applied in fact or in name only in 1940.) Among data at hand, this is the most comprehensive age-class system which is described in detail. It will be noted that there are only four age-classes which bear special names and that the household masters, as such, do not appear to comprise a named, formal class. A free translation of Sakurada's account (pp. 90–92) follows:

As an example of community work allocated according to an age-class system, I cite here the fishing village of Kunizaki, Mie Prefecture, as quite typical. There the life of a man was divided into the following six stages:

1. *Childhood and boyhood*: Until attaining young manhood, children did none of the village work. According to the villagers, this class was without duties. In other villages there are traces of children's associations, but in Kunizaki there is no such group system.
2. *The Young Men's Association* (*Wakamono-gumi*): At New Year's or at the *Bon* Festival of his sixteenth year a boy entered the young men's circle. Regular meetings of the Young Men's Association were held on January 18th and August 18th. At this time, after representatives had worshipped at Aomineyama Shrine, amulets were distributed to all young men and new youths were admitted. The Young Men's Association did the rescue work in disasters at sea and other village duties, and also took care of the stage for the village play. After marrying, a young man retired from this association.[3]
3. *The masters of households*: There is no special name, and I do not know the details of the village service of this class.
4. *Middle Age* (*Chūrō*): After a son had matured and taken a wife, the father joined this group. Until about sixty years ago the middle-aged took charge of funerals. They remained in the class until after their sixtieth birthdays, when they retired.
5. *The Group Without Work* (*Yakunashi*): After retirement from the *Chūrō* and until attainment to the *Tairō* there were no community duties.
6. *The Great Age* (*Tairō*): One entered the *Tairō* at the age of sixty-three. The work of this class was divine service and was comparatively heavy. As the *Tairō* were engaged in religious tasks, they avoided every source of uncleanliness and did not even attend funerals.

It is not certain whether similar duties allocated by age existed for women. There were, however, the classes *Chūba* (Middle-aged women) and *Ōbaba* (Great-aged women). The *Chūba* had no community tasks, but the *Ōbaba* executed the divine tasks of offering prayers at funerals. From this it can be assumed that women also were allotted tasks according to their age.

Systems similar to the foregoing may also be found in northern Kyushu.

Speaking in general terms of age-classes, it can be assumed that the highest class with regard to community duties was that assigned divine service to the tutelary diety. Generally, there existed such classes as the *Tairō*, the *Chūrō*, the *Shukurō* (Experienced) and the *Wakamono* (Youths). The youth's group was the largest and most vigorous among all the age-classes and did its labor under the direction of the elders.

Sakurada also reports on age-classes associated with the division of labor found in communities among the islands of Kagoshima Prefecture, where two named age-classes of males existed. One of these classes was composed of persons from the ages of fifteen to fifty (to sixty in some communities) and the second of older persons (Sakurada: 89). This of course implies the recognition of a third class of male children up to the age of fifteen.

OLD AGE

The only concrete data available on formalized classes for the aged are those given above. There are, however, many indications of the recognition of old age as placing one in a distinct age and status category. The traditional age of retirement from active work in Japan is sixty-one—although few persons of present-day Japan actually retire from active labor until they are well into their sixties. Traditionally, one is allowed again to wear the brightly colored clothing of childhood after reaching the age of sixty-one, and parties are still given in honor of parents and grandparents who reach this age. Such "parties" may also constitute religious ritual. The sixtieth year is one of the several years of life commonly believed in both rural and urban Japan to be calamitous years (*yakudoshi*) for the individual. Ritual is often observed by all members of the household at the beginning of a *yakudoshi* of one of its members to avert misfortune. Observances at the end of the sixtieth year (the beginning of the sixty-first year) ceremonially mark the termination of a period of danger and entry into a new and felicitous period. The old have special privileges in deportment. A grandfather or grandmother may use with impunity terms of speech, including obscenities, considered improper for younger persons. Although these practices are now rapidly vanishing, the privileged behavior of the aged is still strongly evidenced in isolated rural communities, where older women may at parties do lewd dances and make sexual jokes, behavior inacceptable if indulged in by young women (Embree: 172).

As suggested by Sakurada, it seems probable that the major community duties of the aged in a time gone by were those connected with religion. Lingering traces which seem indicative of such an assignment of duties remain in many communities. In Takashima, for example, it is most frequently the household grandfather who makes the numerous offerings to the souls of departed ancestors during the three-day annual Buddhist *Bon* Festival, and he is usually also responsible for all ceremonial preparations. It is customary for the grandfather, who has borne the brunt of the religious tasks during *Bon*, to take a day of rest from all activity when the festival period has

ended. The reading of Buddhist sutras at times of funeral masses is usually done by either aged men or aged women. In many households of Takashima the tasks of making daily or intermittent offerings before the several shrines in the dwelling to household gods are performed by the grandfather or grandmother. This may be in part a reflection of the waning belief among younger persons in the traditional deities. In former times it was a common practice for members of Japanese ruling families, including emperors, to retire during old age or earlier in life and enter the priesthood. (It must be noted, in the case of emperors at least, that retirement was sometimes forced and relatively early in life.)

In summary, it appears that four major classes reflecting age and marital status were generally recognized throughout Japan in former days: infancy and childhood, youth, adulthood, and old age. Additional subdivisions of these major categories based upon considerations of marital status and attainment to the status of household head also existed, and there appears to have been considerable local variation in these classes. The most persistent survivals of ceremonial connected with the taking on of age are those at puberty or at the age of recognized majority, and upon reaching the age of sixty-one, the traditional age of retirement from active life. Formalized, named age-classes for young men and young women appear to have been general. Other institutionalized age-classes were at least not uncommon, but they appear to have exhibited greater local variation. Age-grading appears everywhere to have been closely associated with the division of community labor. In communities which practiced communal subsistence labor such as joint fishing, a division into three age-classes appears also to have been common—children, able-bodied adults, and the aged. In general, there is much less evidence of the present or previous existence of age-classes for females than for males.

In terms of Japan as a whole, clear-cut, general practices of age-grading no longer exist—the *Seinendan* forming a questionable exception. The influence of old practices of age-grading and their associated behavior patterns is, however, evident in a number of customs and attitudes. As many foreign observers have noted, there is a great emphasis upon age in modern Japan. The present *Seinendan* appear to be in large measure an outgrowth of the former youths' associations, and, as noted, in some instances there is historic record of the conversion. The present-day women's societies (*Fujinkai*) found in most communities may also represent a reflection of old customs of age and status grading. The *Fujinkai*, like the *Seinendan*, were for many years nationalized and government-bolstered organizations encouraged for the promotion of nationalism. Today both of these organizations may be described as civic clubs whose membership is based upon age and marital status, as well as presumptive common interests.

Linguistic evidence also indicates the emphasis upon age. All old persons including strangers are, for example, customarily referred to and addressed by

younger persons as *ojiisan* and *obaasan* (grandfather and grandmother) and middle-aged persons by terms translatable as uncle and aunt. This also suggests an old kinship system with kin terms on a generation level. These terms, in former times at least, bore connotations of respect.

The colors of clothing remain closely connected with one's age. During infancy and childhood colors are bright and they become progressively somber as one approaches old age. The respect and deference paid to age have been and are still evident in politics and government. During feudal times under Hideyoshi (1536–1598) a five-member council of state was called *Go-tairō*, "The Five Great-aged." The first minister during the Tokugawa period (1615–1868) was called variously *Go-tairō*[4] or *Ō-toshiyori*, "The Honorable Great-aged One," and *Genrō*, "Founding Aged-one." The senate created in 1875 after the opening of Japan to contact with the West was called *Genrō-in*, "Founding Elders." Although this senate was soon abolished, a group of mature statesmen known as the *Genrō* (usually translated as "Elder Statesmen") remained of great influence in governmental affairs for many years (Sansom: 364).

A study of local customs would no doubt reveal many which were once associated with age-grading customs. It is suggested, for example, that the custom observed widely throughout Japan today of the transporting of portable shrines (*mikoshi*) by young men during festivals is a reflection of the former assignment of community tasks to the young men's association (Sakurada: 89). In the community of Takashima, to cite another example, an annual ceremony whose origin and meaning are now unknown is observed by all unmarried youths of sixteen years and upward. Contributions of money are made by each household of the community for the purchase of rice wine for a party for the young men during the annual festival of the local tutelary god. Young men participate in this drinking party for the first time during the year that they reach the age of sixteen, and on this single traditional occasion of the year a young man may get drunk without parental censure. Although no youths' age-class except the recently established *Seinendan* is known to have existed in the community within the memory of the eldest living persons, this ceremonial has always been observed. It appears plausible that this ceremonial represents a survival of former puberty rites or of combined ritual for puberty and for initiation into a young men's age-class.

The foregoing are but a few examples which may be found in modern Japan of the emphasis upon age and the lingering customs related to former practices of age-grading. A thorough study of Japanese customs and attitudes from this viewpoint gives promise of being of aid in interpretations of present-day Japanese culture.

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NOTES

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[2] What I have here translated as "southern Japan" and "northern Japan" are the Japanese terms *seibu*, "Western (Japan)" and *tōbu* "eastern (Japan)," respectively.

[3] This leaves unexplained, or at least unclear, the interval between marriage for a young man and the attainment to mastership of a household. The eldest son does not normally become master of the household until the age of forty or over, when his father has retired because of old age.

[4] The terms *Go-tairō*, as applicable to the officials under Hideyoshi and to the Tokugawa first minister, are homonymous but the first element (*Go*) differs in meaning and is written with a different ideograph.

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Kinship Changes at Caughnawaga[1]

By FRED VOGET

INTRODUCTION

THE current settlement at Caughnawaga, Quebec province, in large part is the end-product of Catholic missionary activities dating back to initial contacts with the Iroquois prior to the mid-seventeenth century. The present site is the fourth of a series initiated at La Prairie, Quebec, in 1667. The Caughnawagas maintain a traditional descent from the Mohawk tribe, and today the Mohawk dialect is spoken in a substantial number of homes. (For a history of the mission settlements, see Devine, 1922.) The village site is but eight miles from metropolitan Montreal, and the inhabitants thus have been subject to heavy acculturation. Moreover, substantial miscegenation with white nationalities has resulted in a predominantly mixed population. Presently the majority of the able-bodied men are engaged in steel erection work, where they are known as expert riveters.

The purpose of this preliminary excursion into kinship at Caughnawaga is to trace in summary form the changes through which the kinship system has passed before arriving at the current Euroamerican subtype of the Eskimo system, and, at the same time to attempt a review of the possible causes for these developments. The data are drawn from two main sources: L. H. Morgan's list of kinship terms which appeared in the "Systems of Consanguinity and Affinity of the Human Family," and field work undertaken by the writer in 1949 and 1950.

IROQUOIS KINSHIP SYSTEM

Morgan gathered his data among the Iroquois from 1859 to 1861, and his lists show some striking differences between the Seneca and Tuscarora on the one hand and the Mohawk, Cayuga, Oneida, and Two Mountain (Oka) Iroquois on the other. It is quite apparent that Morgan considered the Seneca to represent the ideal or original system, for the kin terms of this tribe formed the basis for his distinction of the Ganowanian Famliy. Contemporary writers, such as Spier (1925: 77–79) and Murdock (1949: 244) are in essential agreement, the latter referring to the Seneca type as the "normal Iroquois" system.

An examination of Seneca terms reveals the importance laid upon the *sex of the connecting relatives* (bifurcation) in the classification of relations. Where the sex of the connecting relatives is the same, collaterals tend to be equated or merged; where the sex is opposite, the collaterals tend to be kept distinct. Thus, a father's brother and a mother's sister are referred to as father and mother and their children are considered brothers and sisters. In contrast, a father's sister and a mother's brother are referred to as aunt and

uncle and their children are considered cousins. Here we have the basic structure of the Iroquois kinship system as defined by Morgan (1871: 131–166), Spier (1925: 77–79), and Murdock (1949: 223). However, the importance of the sex of connecting relatives also may be traced to the first descending generation, where children of a brother (woman speaking) are referred to as nephews and nieces, while the children of a sister are sons and daughters. The same method of classification is extended to the children of cousins and classificatory brothers and sisters, where we find the children of male cousins are classed as nephews and nieces and the children of female cousins as sons and daughters. The extension of a nephew-niece and son-daughter terminology to the children of cross-cousins may indicate an incipient generational shift.

The second important criterion to be found in the Iroquois system is that of *generation*. The results of its application are to be found in the second ascending and the second descending generations, where individuals are classed as grandparents and grandchildren respectively. This criterion also supports that of the sex of connecting relatives where merging takes place, and conflicts where a collateral relationship is defined. *Relative age* is of importance to the Iroquois system only in respect to the immediate relatives of Ego's generation, specifically to those referred to as brothers and sisters, whether real or classificatory.

The social structure which supported the above kinship system was based on an agricultural-hunting economy, supplemented with collecting and fishing.[2] The conjugal family was merged with a larger unit, the matrilocal extended family. The basis for this extended family was a female lineage, the members of which tended to inhabit a single longhouse or several in close proximity.[3] While the prevailing tendency was matrilocality, a degree of variability seems to have occurred, instances of patrilocal residence are implied by certain authors, and the Seneca are reported to have followed the latter form for the most part (Quain 1937: 256, Citing material from William N. Fenton). Two or more matrilineal lineages comprised a matrilineal sib (Goldenweiser 1922: 74; 1914 b: 368–370). A dual alignment of the sibs also seems to have occurred in each tribe, these units technically being referred to as moieties.[4]

MODIFICATIONS

Morgan's lists disclose that changes had already begun among the Mohawk, Cayuga, Oneida, Onondaga, and Two Mountain (Oka) Iroquois of his day. This we may term Modification 1. The innovations pertain to the terminology employed by women and are two in number: the father's sister is referred to as mother, and the children of brothers and sisters (real and classificatory) and cousins alike are considered sons and daughters. The mother's brother still is referred to as uncle.[5]

In contemporary Caughnawaga the oldest extant system employed by

both sexes is based on a generational classification of relations, so that members of the first ascending generation are referred to as parents, members of the Ego generation as siblings, and members of the first descending generation as children. This system, Modification 2, variant *a* was reported by a conservative middle-aged couple. The informants also stated that the system was in use among a number of their relatives, and that some fifteen years ago a rather large number of "old people" classified their relations in this wise. At another time the same female informant gave a second variant (*b*) in which the parental collaterals were distinguished as uncles and aunts, cousins were referred to as brothers and sisters, and the children of the latter were classified as nephews and nieces. Members of the second descending generation were referred to as grandchildren. As will appear below, it is quite probable that the parental collaterals were not always distinguished so fully, but that the father's brother and mother's brother may have been referred to as uncles and the mother's sister and father's sister as mothers. It is also quite likely that the grandparents of the present generation made use of any one or all of these variants, in much the same manner as contemporary informants, who may make use of one or more related variants.

Modification 3, consisting of two variants, represents the basic classification of relations among the more conservative and older members of Caughnawaga and Oka. In variant *a* Ego (man or woman speaking) distinguishes the father's brother and mother's brother as uncles and merges the father's sister and mother's sister with mother. The descendants of the parental siblings are classified as cousins, while the offspring of cousins and own siblings are nephews and nieces. The second ascending and descending generations are distinguished as grandparents and grandchildren respectively. A few informants reported the distinction of the father's sister and the mother's sister as "second" mothers. In variant *b* the full distinction of aunts and uncles brings the system into basic alignment with the Eskimo system as currently defined. Relative age still applies to own brothers and sisters.

Modification 4 again comprising two variants, represents recent shifts with respect to the descending generations. Several informants attested to the recency of the innovations, placing them within the last 25 years or so. Variant *a* is the same as 3b except that the nephew and niece terminology is carried into the second and third descending generations, Ego's own children and grandchildren excepted. In variant *b* the first and second descending generations are considered grandchildren, own and siblings' first generation descendants excepted. The children of a brother or sister are referred to as nephews and nieces, and their offspring in turn are grandchildren. Both Caughnawaga variants were confirmed by Oka informants.

With modification 5, Caughnawaga and Oka kinship terminology approaches the "popular" Euroamerican type. The term cousin is applied in

depth to succeeding descendants of cousins; references to a second or a third cousin may be made in English or by a direct translation of the English into Iroquois. However, some minor differences exist. While the children of siblings are referred to as nephews and nieces, their offspring in turn are usually referred to as grandchildren.[6] When a degree of specificity is required, the speaker may refer to the subject as "my nephew's son" or may use a descriptive terminology, such as "my brother's son's son." While there is a tendency to recognize the collaterality of paternal and maternal grandparents, this is not the customary usage. As a matter of fact, distinction can only be made by means of a specific reference, as, for instance, father's father's brother, or by the use of English, for no Iroquois term has developed.

DISCUSSION

Without entering into a meticulous consideration of the terminological shifts summarized above, it is evident that the Caughnawaga Iroquois system has passed through two major series of changes, based in the first place upon a criterion of generation and in the second place upon that of collaterality. Owing to a lack of adequate statistical evidence, it is not possible at this time to determine with certainty the effective causes for the changes observable in the Caughnawaga kinship variants, but some suggestions may be given. As Murdock (1949: 199) and Spoehr (1947) have demonstrated, the source for these changes must be sought first of all in the "altered life conditions."[7] Some of these changed conditions derived from adjustments which the Caughnawagas made to modified economic conditions; others were probably stimulated directly by white pressures of a religious and administrative nature.

As the basic subsistence unit, the family, whether extended or nuclear, appears to be especially sensitive to changes in the economic pattern. Since first contact with Whites, the Caughnawagas have been caught up in some aspect of Euroamerican economy. The Caughnawaga male, first as a trapper, then as a farmer-lumberjack-bargeman, and finally as a steel erection worker, has gradually emerged as a relatively skilled and highly paid workman in a specialized segment of the industrial economy. The trend, then, has been toward a pattern which emphasized the productive role of the male and a diminution in the contribution of the female. It is significant, however, that the economic activities of the male have been characterized by a high degree of mobility, and it is only within the last forty years that wives and children have accompanied workers to any large degree. Employment in the United States has allowed the establishment of independent household units, and "little Caughnawagas" are reported for Brooklyn, Detroit, and Buffalo.

Another aspect of this work is its seasonal character, allowing some months of inactivity, especially during the winter. Such conditions would

appear favorable to bilocal as well as matrilocal and patrilocal arrangements; and a number of informants, 50 years and older, have reported households of two to four conjugal units including that of the parents. The accidents of housing and affluence have affected the arrangements, and it is probably best to refer to a bilocalism rather than a matri- or patrilocalism. It would seem, too, as if the larger bilocal arrangement began to give way by the turn of the century to a smaller bilocal unit based increasingly upon temporary residence. Under this arrangement the household might consist of the parents, or a widowed parent and a married son or daughter as expediency might dictate. In some instances married couples have succeeded to the dwelling upon the death of the parent, or, after a temporary stay, they have elected to construct their own home upon a plot of common land held in restricted title by the male head. Again, according to the accidents of inheritance, the newly married couple may build on land adjacent to the parental dwelling.

While to all intents and purposes such families may be treated as simple conjugal units modified by the presence of one or more parents, it is important to note that the majority of informants contacted had a record of either such a temporary matrilocal or patrilocal residence. The trend toward neolocality, then, has been a steady one, but the process has been gradual and tempered by temporary bilocal arrangements. As the productivity of the male has increased, and as he has been able to include his own family in his work arrangements, the neolocal trend has been greatly stimulated. The development of a separate household arrangement also has been stimulated by religious teaching and administrative policies, where we find the male status and role defined according to the Euroamerican pattern.

The effects of bilocalism with regard to kinship are to be found in the potentialities for certain relations to be brought into a functional socioeconomic unit; for such a system of residence may bring both maternal and paternal collaterals under one roof or in close association and thus lay the basis for an extension of kinship terms along generation lines (Murdock: 152, 208–09). The effect of increasing separate residence allows the emergence of a new type of socioeconomic unit, the conjugal family, and should result in the equivalence of the households of collaterals. However, in contrast to the equivalences which arise on the basis of propinquity and socioeconomic cooperation characteristic of bilocal arrangements, the equivalences associated with neolocal residence emphasize the uniqueness of the collateral households. In this way a trend toward collaterality should set in and a lineal terminology emerge (Murdock: 152–153). A further consequence of the emerging neolocality is a shrinking of interaction with relations. Currently the Caughnawaga adult knows few relations beyond a limited segment of the bilateral kindred with whom he happens to be on more or less intimate terms.

While the above, following Murdock, serves to explain the processes that

resulted in trends from bifurcation to generation and finally to collaterality, the order of change according to the particular generation deserves comment. An examination of the original Iroquois system discloses that three generations were functionally significant and therefore to be distinguished, namely, Ego, first ascending, and first descending. The socal significance of individuals in both the second ascending and descending generations apparently was of less importance or at least equivalent, for these were grouped according to a generalized term.[8] Throughout the various modifications to which the Iroquois system at Caughnawaga has been subject, a terminology based on generation for members of the above generations has been maintained, Modification 5 excepted.

Turning to the functionally significant generations, it is noteworthy that the mother's brother's status initially resisted the generational trend (cf. Spoehr 1947: 204; Hallowell 1928: 126). The persistence of this status probably was related to the maintenance of special functional relations between a man and his sisters' children owing to a continued emphasis upon succession to sib membership, names, and certain titled properties through the female line. When this status reappears, following its "submergence" by the generational trend, it is in association with that of the father's brother.[9] The first status to be affected was that of the father's sister, who in Morgan's day was equated with mother and mother's sister. The current practice of conservative members of referring to the above individuals as mother, while distinguishing the collateral males as uncle, may derive in part from the increasing importance of males as household heads. It is not unlikely that the relative propinquity of the female relations in contrast with the mobility of male relations may also have contributed to the equating of mother, mother's sister, and father's sister and the distinction of father, father's brother, and mother's brother. A further point of interest is the term for aunt, which is based upon the French *tante* and is the only form disclosing direct Euroamerican influence.[10] No informant has been able to recall the native term for aunt.

The Ego generation discloses the interesting fact that changes in terminology for this generation followed upon realignments, either in the first descending or first ascending generations. For example, the classificatory shifts in the Ego generation from a parallel- and cross-cousin to a sibling terminology followed prior changes in the first descending and was probably coterminous with similar changes in the first ascending generation. When the trend toward collaterality sets in, terminological shifts in the Ego generation appear to be articulated with similar changes in the parental generation.

The first descending generation seems to have been especially sensitive to changed conditions. The most striking shifts recorded by Morgan (1904 I: 312) appear initially in the first descending generation, where we find a woman referring to the children of brothers, sisters, and cousins alike as sons

and daughters. An explanation for this initial change may lie in part in the increasing importance of the emerging bilateral kindred as a regulator of marriage. A useful way to implement this control would be through an extension of incest taboos, and in the present instance the impact of Euroamerican marriage practices upon the pronounced tendency of the Iroquois to marry someone a generation removed may account for it. While bilocalism in large part may account for the generational shifts in all levels, it should be noted that these terminological changes were in accord with marriage practices of the dominant society. The extension of a terminology implicated in prohibitive marriage regulation would be one alternative; the other would be complete acceptance of the new terminology together with the necessary redefinitions of status. From what we know of cultural change, it is probable that the former alternative has been more usual during initial periods of contact.

The recent application of a nephew and niece terminology to descending generations in depth probably represents an increasing awareness of Euroamerican classificatory practices with reference to own siblings' children and their descendants but more especially with reference to cousins and their descendants. The extension of a terminology in depth, across generation lines, accords with a classificatory principle employed by the Iroquois in relation to functionally non-significant generations, that is, relations beyond the grandparent and grandchild generations. Of special interest is the partial reverse of this tendency through the application of a grandchild terminology to the children of cousins and their descendants. Both variants may be viewed as incidental to the final adjustment to the Euroamerican system, and probably are indicative of a diminution in significant interaction between kindred relations. However, a number of informants have suggested the opposite—that these usages are attempts to emphasize a degree of relationship which in fact does not exist.[11] Support for this contention can be found in the uniform practice of referring to older people as uncle and aunt and grandfather and grandmother irrespective of their relationship. The usual explanation for this practice is based on a notion of respect. In like manner, older people may refer to children irrespective of their relationship as grandchildren. Such practices emphasize the biological relationship of the Indian *vis-à-vis* the White, and it was evident in the course of the investigation that a substantial number of the informants idealized this relationship. Individuals for whom the sib is still important greet their sibmates as brothers and sisters, even though these be from another Iroquois tribe. In like manner, the members of the Handsome Lake worship may greet each other as brother and sister. It is not unlikely, then, that in the above variants, which are immediately transitional to the Euroamerican system, we have the composite effects of a number of group arrangements based on differing principles of organization—all of which, however, emphasize group solidarity. The extension of the nephew and

niece terminology in depth also may represent a terminal phase in the emphasis upon the kindred as a functional unit.

Some "lags" in adjustment also are found in the classification, particularly with reference to affinals. Thus, users of both Modifications 3 (*a* and *b*) and 4 (*a* and *b*) refer to the spouses of cousins as brothers-in-law and sisters-in-law. In like manner, the spouses of nephews and nieces are considered daughters-in-law and sons-in-law. Those who follow Modification 3*a* also refer to an uncle's spouse as mother, whereas the spouse of an aunt is uncle. The relative age of siblings, which finds some support in Euroamerican practice, is maintained throughout all modifications. The sex of the speaker, a criterion of importance to the native system, is restricted to one usage, a distinctive term for brother-in-law existing for each sex.

SUMMARY AND CONCLUSIONS

A review of the Caughnawaga and Oka data seems to allow the following summary and conclusions:

1. The type of family organization, based upon residence and socioeconomic functions, is significantly related to the kind of kinship system developed. This is evident from the emergence of a generational type of terminology at Caughnawaga, presumably in association with bilocalism. With the emergence of the nuclear family as the basic subsistence unit, a trend toward collaterality has set in, and a basic Eskimo system has developed.

2. Change by subsitution of the terminology of the contact society for that of the native society is not in evidence in the initial phases of change. Rather, adjustments have been achieved gradually through a series of modifications, a number of which are usually in operation at one point in time. It is possible, however, that the native system has been influenced indirectly by the imposition of a legally supported marriage system.

3. There is an implication that changes have been effected through a functional redefinition of status types and roles owing to changed conditions of life, and this may account for the sequence of terminological change with respect to the several generations. There is also a suggestion that emphasis upon group solidarity has contributed to status terminology in the later modifications. Some terminology, applicable to a previous status-role definition, is still retained, particularly with reference to affinals.

4. No new principles of classification have been introduced in the course of the modifications to which the Iroquois system at Caughnawaga has been subject. Rather, certain principles have been accorded a new importance, and others, formerly of great significance, have been restricted in their application or completely lost, as in the case of bifurcation.

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NOTES

[1] This report is a portion of a broader investigation financed by the Canadian Social Science Research Council. The writer wishes to thank Dr. G. P. Murdock for helpful criticism and insight into the problems of social structure and process. I am also indebted to Henri Bechard, S. J., of Caughnawaga, who generously offered the use of Pére Marcoux' Dictionnaire Français-Iroquois. This paper was originally presented at the meeting of the American Anthropological Association, Berkeley, 1950.

[2] Quain (1937: 241) accepts the statement of the Jesuit Bressani that the Iroquois hunted only for pleasure. While the trapping and war activities of the male in relation to the fur trade may have contributed to a decline in the male contribution to the immediate subsistence, to assume that hunting was for recreation only would seem to underestimate the significance of this activity in relation to the total economy. Kroeber (1939: 146–150) is of the opinion that agriculture yielded no more than half the subsistence, hunting, fishing, and collecting contributing the remainder. He would define the Iroquois as "agricultural hunters" rather than outright "agriculturists".

[3] Goldenweiser (1922: 73) describes the female descent group as comprising "a head woman or matron, her immediate male and female descendants, the male and female descendants of her female descendants, and so on." Goldenweiser refers to this as a maternal or matrilineal family, and is so followed by Quain (1937). However, the membership obviously corresponds to what may best be described as a lineage.

[4] Goldenweiser (1914a and 1922) usually referred to these unnamed units as "phratries".

[5] The Dictionnaire Français-Iroquois compiled by Pére Marcoux when resident at Caughnawaga (ca. 1835) allows a limited reconstruction of the system then in use. It would appear that the Caughnawaga system was like that recorded by Morgan for the Two Mountain Iroquois, except that males as well as females referred to the father's sister as mother.

[6] Quimby (1939: 561) has recorded a similar treatment of the children of nephews and nieces among the Potawatomi in Michigan.

[7] Generational trends in the classification of parental siblings among the St. Francis Abenaki were recorded by Hallowell in 1928, a development which he considered baffling insofar as he could discover no sociological datum which might serve as an explanation. It is evident, however, that Hallowell considered the presence of a unilateral organization the *sine qua non* for the emergence of classificatory trends (Hallowell 1928: 142). Generational trends are also in evidence among the Penobscot and Malecite (see Speck 1918 and 1920; also comments by Hallowell 1928: note 116; 118–119).

[8] This implies that a generational terminology may also result from a lack of socioeconomic importance. Murdock (1949: 136–137) refers to this process of equalization as "immateriality".

[9] A similar terminological extension occurred among the Potawatomi of the Huron in Michigan and the St. Francis Abenaki (see Quimby 1939: 560–562; Hallowell 1928: 126).

[10] One youthful informant who had been educated for the most part in French parochial schools outside Caughnawaga employed a term for uncle based on the French, *oncle*. Similar usage was reported for other young people of both sexes.

[11] Such a statement, of course, could only derive from more acculturated informants who accept the definition of interpersonal relations found in Euroamerican society.

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SYMPOSIUM: DO WE NEED MORE BECOMING WORDS?

This is the first of a series of symposia the object of which is to expedite scholarly intercommunication. Professor Count's query was referred to several colleagues for comments which were sent to Professor Count for rejoinder, then the series was edited as a whole. Similar queries in any field of anthropology, or at the margins, are invited, and the author may suggest names of persons whose comments he would value.

QUERY *by* EARL W. COUNT

If lexical content of a language reflects the thought of the culture of which it is a part, then semanticists and anthropologists alike, if they are minded to philosophize about culture history, might meet over the proposition that the English language (like others) is far richer in words denoting state than in words denoting process. The proposition is herewith submitted.

We have plenty of words for things that are, or are conceived to be, but few that denote *becomingness*. It happens that I am writing a book on the evolution of human sociality out of its vertebrate matrix. Repeatedly there is occasion for a word which, for all my search, simply is not. At this point, my letter becomes a "Words Wanted" advertisement.

Here are some concepts for which words are wanted:

(1) "Becomingness" itself. The process or events by which something comes into existence. Synonymously, or by corollary.

(2) To *attain*, to *achieve* existence.

(3) To *evolve*. For instance, the human brain and mind evolve. But it is not mere alteration. There comes into existence something that previously had not existed, by a process-behavior that analogizes with the "unfolding" of a bud when it becomes a blossom. The word I am seeking should contain the idea of antecedent properties that demonstrated a potential.

(4) To *render* precise. Can someone suggest something better than "to precision"?

(5) The *becoming* human (like Spanish "hominación"); particularly in the evolutional sense.

(6) "Chronopoesis." Suggest something better for that event or process which, perhaps more than any other single mental happening, by evolutionary course has achieved that *hominación* of a primate *modus vivendi* which we call "culture": the social reification of time; the "time-binding" of man's way of living, which makes kinship and tradition possible.

(7) "Valuation." Wanted: a word denoting the *process* of creating value itself. Also, a word for the process of creating values, or of attaching value to something. Again a fundamental point of cultural distinctiveness, an essential in the process of *hominación*.

(8) To *become* akin to. As, when one family makes marriage alliances with another; or, blood-brotherhood is bonded; or, adoption occurs.

It would not be hard to expand the list. The significant fact about all this is, that as you read these sentences, you know what I am talking about. A couple of centuries ago, you would not have. Somewhere in between, there has been passage from the one condition to the other. Again to speak by parable—Euclidean geometry and Arabian algebra had no room for analytical geometry and the calculus of variables and infinitesimals. But between those achievements, the thought of a culture had changed. The geometry of movement and the algebra of flux emanated not from the Descartes, the Leibnitzs, the Newtons alone, but from the culture-world which gave these men being.

Do we not already, perhaps even without realizing it, add the dimension of time into our conceptions of state, so that we think of them as more or less instantaneous or transitory phases of a fluid situation, which has its antecedents of becoming and which will have its "*post-cedents*" in the future? Are we not struggling toward a conception of culture and of man as something *happening*, not simply *being?*

If this calculus-way of looking at states ever suffuses not only our lexicon but also the thought-processes and thought-habits of society, may we expect that the cultural stand-patter will look a little quaint?

Comments

by Eliseo Vivas:

In a very few words Mr. Count has managed to pose enough problems to keep a whole college of scholars busy for months.

Let us look into some of the terms he advertises for. If the term "to evolve" cannot be stipulated to refer to a process through which "antecedent properties demonstrate a potential," why not "to emerge?" I recommend this term highly. It happens to be doing magnificent work in naturalistic philosophy at present for the very notion that Mr. Count seems to have in mind: a process which is teleological but which we must not be caught admitting in so many words to be such. And if what is needed is one word for "to attain existence," and the millennial veterans "to actualize" and "to realize" must be retired, and an odd-sounding new term must be commissioned in their place, I suggest "to existentiate." Ditto for "to become akin," for which I suggest "akinify." These two new terms seem to me to have all the qualities that would endear them to Mr. Count and would enable him to precision his thought; indeed I would say they both are beauties. Such terms as "to humanize," and "to valuate," should also help to improve our thinking and to make it scientific. But the term "chronopoesis," I suggest, stands in a class by itself; were we to put it with the others, it would stick out like a lily out of a heap of cacophonous neologisms.

My second point is that Mr. Count confirms a fear I have had for some time that anthropologists—perhaps because they have akinified with their

colleagues in psychology and sociology—have caught a delusion the latter have long been suffering from. For this disease I suggest the term "logophilia." It manifests itself as a euphoric feeling on the part of the victim when he invents a neologism, particularly if it is a cacophonous one, and in a conviction that the invention precisions his thought.

Take for instance Mr. Count's assumption that "man makes value." In this conviction he is aided and abetted by the great majority of contemporary American philosophers. But a majority is not enough to transmogrify a mere article of current ideology into a verified truth. Whether values are made or discovered is a difficult philosophic problem which the social scientist cannot resolve with the means available to him. I suggest that he need not resolve it in order to accomplish anything he can legitimately tackle in the field of his competence.

Logophilia has another unfortunate consequence. One of the things on which American anthropology can plume itself is the number of distinguished scholars whose contributions gained in quality, as it seems to an outsider, because those who made them had a sense of the elegance and flexibility with which the English language could be used. Think of Sapir, Linton, Redfield and Benedict. The English language, with a moderate admixture of technical terms chosen prudently and with good taste, has until now served anthropologists in a very adequate way. It could continue to do so if anthropologists do not allow themselves to be lured by the delusion that multiplication of uncouth terminology is equivalent to clear thinking.

Nor is it true that we have only recently begun to appreciate the fluidity of existence. Heraclitus has been with us as long as Parmenides. And our language has long expressed the aspects of the flux its users found necessary to express. Must we now substitute for the qualities which the language of Sapir and the others brought into anthropology an uncouth and pseudo-scientific bafflegab?

by JAMES SLEDD:

If I understand Professor Count, he argues from two main propositions. First, the things that people talk about show us how and what they think; and second, the English language "is far richer in words denoting state than in words denoting process." He seems to conclude that until "a couple of centuries ago," the English-speaking world thought of man and of man's culture as things which are, not things which are happening. Professor Count, as a representative anthropologist, does not think in this fashion. He is struggling to express the new conceptions of man and society in English terms of process, and so it follows, for him, that the English-speaking world is changing. A new mode of thought is emerging, and the conservative, "the cultural stand-

patter" with the old vocabulary and the old ideas of state, may come at last to look "a little quaint."

It may be that Professor Count is right. His speculations are a long way from the areas in which I feel at home, and I claim no real competence to judge them. Since you have asked me, however, to express an opinion, I must say that I can accept neither his evidence nor his reasoning. I think he is simply in the situation in which we have all found ourselves, at one time or another. He wants to write a book, and the words won't come. He wants to change the world, and the world won't change to suit him. Whether man is a happening or a being, we know what a man will do in this state (or process): he will conclude that the English language is defective.

In this diagnosis I am perfectly serious, and I hope I can show cause why it should not be treated as a piece of gratuitous rudeness. To begin with, I am very sceptical of facile reasoning from the vocabulary of a language to the thought of the people who speak that language. Some just conclusions may no doubt be founded on such reasoning. The absence of certain terms from my vocabulary might be used to show, for example, that from 1915 to 1935 the middle-class white Protestants of Decatur, Georgia, were not deeply concerned with fly-fishing, with a priestly hierarchy, or with alchemy; and this was indeed the case. Conclusions of this sort, however, based on facts which can be precisely stated, and limited to simple judgments about definite groups at particular times and places, are very different from sweeping inferences concerning all the speakers of a great language, or some large but unspecified group of speakers, over a period of several centuries and a vast but undefined geographic area.

When I examine that argument, I wonder what real evidence Professor Count may have that English is actually lacking, as he says, in "words denoting process" but rich in "words denoting state." He wants a name for "the process or events by which something comes into existence." If he would specify the kind of process of becoming which he wants to name, I should think that any good dictionary or thesaurus would provide him with some dozens of terms; but, for a start, and without such specification, one might propose *becoming*, *beginning*, *birth*, *commencement*, *development*, *emanation*, *emergence*, *evolution*, *formation*, *generation*, *genesis*, *growth*, *inchoation*, *incipience*, *nascence*, *origination*, etc. Professor Count also wants a verb meaning "to render precise"; he suggests *to precision*. I think that *to precision* will give as much offense as *to suspicion* used to give my high-school English teachers, particularly when *to render precise*, or *to make precise*, is so obviously available. If we must have a single word, I might risk *to precise*, which had at least some currency in the nineteenth century; but finding the word has not precised my notion of precision. Generations of scholars have had a very clear,

immediate, and painful understanding of many processes for which they have no name; I think daily of the process by which my chairs and rugs wear out as I try to improve the furniture of my mind.

by DOROTHY LEE:

In answer to Professor Count's letter, I would like to say that I believe the English—at any rate the American—language does have the kind of words he wants, in a rapidly increasing number, but that these are not available for academic usage. Such is the word "to precision" which he rejects. Such also are words which the school does not consider appropriate in writing, but which do include process and the time dimension, formed through the addition of suffixes such as *-out* and *-up*. *To think-out* stresses the careful process of thinking which arrives at a definite result; *to think-up* introduces spontaneity, quickness and again definite result. But we tend to avoid these suffixes in learned writing, because apparently we have learned to avoid them in all writing except fiction or journalism. At any rate, they appeared only occasionally in the high school compositions which I examined. For expressing *becoming*, we have the auxiliary *to get;* in addition *-up* and *-out* function in this way sometimes, and are now even used occasionally with words of Latin origin.

I think the academic resistance is to grammatical processes which have been active throughout the history of Anglo-Saxon, but largely in connection with Anglo-Saxon words. Since words of Latin origin are often preferred in writing or educated speech, these processes have not taken place in academic English even though the thought of the times, as Professor Count shows, demands such processes for its expression. Recently, the need for economy has lowered the barriers among those academic people working in "applied" fields; for example, *to contact* is as acceptable as *to brief*.

What I am trying to say is that if Professor Count was willing to use—and was completely at home in—vernacular American, he might find it more expressive of what he wants to say. I should confess, however, that I would probably not then enjoy reading what he wrote.

by GEORGE GAYLORD SIMPSON:

It is surely true that consideration of change and of time is enlightening and necessary in modern scientific theory. It is a promising approach in anthropology to direct attention to formative processes and not only to their formed consequences. Probably no one will question the wisdom of Dr. Count's remarks on that score, and comment would be amplification more than modification. There may, nevertheless, be some doubt about the quest

for specific terms, presumably single words, to designate concepts of which eight are given as examples.

Dr. Count succeeds well in designating the concepts for which he lacks terms. As his own examples show, the English language does readily designate process as well as state. It does so usually by a generic verb of process followed by a specific noun or adjective of state. Most of Dr. Count's examples provide their own solutions: (1) to become, or if a noun is wanted, the process of becoming; (2) to come into being (or existence); (3) to develop (from earlier bases or origins); (4) to render precise; (5) becoming human (or, but less apt, a literal translation of the Spanish word, "humanization"); (6)—see further remark below; (7) to create or to attach value; (8) to become akin. Such expressions seem to me nearly self-explanatory and sufficiently concise and precise. To replace each by a one-word term would run the risk of developing an obscure technical jargon. It is a fault and not a virtue of science to use special terms for concepts that can be designated equally well in the vernacular.

There are, however, concepts so complex that they cannot be clearly and simply designated in ordinary English. They must be described rather than briefly stated. Dr. Count's example (6) is one of these. Then the description may be given at such length as is necessary, and later reference to the concept may be abbreviated by use of a special term that stands for and is defined by the prior description. Decision to employ such terms depends on balancing the convenience of brief designation against the inconvenience of using an esoteric vocabulary. Technical or otherwise specially defined terms are obviously necessary in every science. Nevertheless they are a barrier to achieving understanding and to becoming understood—basic "processes" as opposed to "states" of science—and it behooves us to employ them only when they do have clearly compensating advantages.

by DAVID BIDNEY:

I find it difficult to suggest words for Count's "Words Wanted" advertisement. One would have to coin words in the manner of Whitehead in his *Process and Reality* and I would suggest that Count consult that difficult work. Whitehead is the philosopher of process and events and has struggled more than any contemporary philosopher with this problem.

Here are some awkward attempts:

1) becomingness:—just "becoming" might do
2) to achieve existence: perhaps "existentialize"
3) to evolve—to "evelope"
4) to render precise—to "precisionize"
5) becoming human—"humanize"—"humanization"
7) "valuation"—"axiopoeic" process; "axiologize"

by F. S. C. NORTHROP:

What Count says about the English language is true and goes back in considerable part to Aristotle's substance attribute terminology. Among philosophers Bergson and Whitehead have been especially cognizant of the problem. The somewhat difficult terminology of Whitehead's *Process and Reality* is one attempt to meet it.

REJOINDER

Apparently my original communication failed to make clear to some commenters two aspects of the essential proposition. I am sorry I did not do better. These two aspects are: (1) The relative paucity of terms sought for, and the handicap this entails. This is the minor aspect. (2) The course of culture-history which this reflects. This is the major aspect. Let us take them up in this order.

As to the list of terms set forth: (a) Circumlocution admittedly is always possible, is always being done. There is no suggestion that the English language is inadequate for rendering even the most advanced thinking in this manner. (b) Attaching a new meaning to an old word is indeed of the very essence of semantic development within all languages I am acquainted with. (c) So is the forging of neologisms. Confronted with an actual problem of expressing something hitherto unexpressed, a writer has his choice of the three devices—and he must take the consequences his readers and auditors visit upon him. It happens that the topic in hand concerns neologisms. For those comments which have taken the problem seriously enough to offer constructive suggestions, I am grateful in like manner and degree; and I regret that space forbids responding to them as they deserve

Surely I need not amplify the statement that verbs and substantives have been coined, since time immemorial, not for the mere purpose of saving print—the process is thousands of years too old for that—nor always merely for the fun of toying with new morphemic combinations; but to make communication smoother. And when does that succeed? A fair question; but it must be tabled.

Shall we refer to one or two of the terms originally tossed out?—To "render" certainly is a hard-working verb, a good man-about-the-house. It steps in and takes care of all sorts of emergencies. Sometimes we give it a rest well earned when we "redden," "nullify," "soften," but do not "render red," "render null," "render soft". (But do as you prefer. If, when we speak together, I defer to your tastes and you to mine, we shall each have a high opinion of the other's tastes.) We are not, however, as considerate of poor "render" ("*rendre*"—mais comme vous voulez) as are the French, who have long offered us their verb "*préciser*"; unaccountably, we have declined to pick it up; we prefer to "render things precise"—which (to my ears, anyway)

is rather self-defeating in that it lacks the convincing overtones of precision. *Sed de gustibus*—

Some of the terms "advertised for" will probably be forthcoming before others. Thus, if the Spanish-speaking anthropologists continue to discuss *hominación*, we may yet be driven to it. It is a good word. I like it. I am sure that the anthropologists who have been studying blood-brotherhood would welcome a suitable term for the process of "becoming akin to"; but this is one term which thus far has subbornly refused to be born.

Now let us turn to the second, and major, aspect.

It would be manifestly untrue to claim that "becomingness" has been unknown to the world until recent times. For instance, every cosmogony ever invented supposes that the present state has come into being over a chain of events interpolated between it and a totally different pristine state. By inverse logic, the pristine state is worked back to, and then the aetiology is reconstructed by facing all the cards in the stack other side up. Not unlike a Euclidean theorem. This leads to two very basic observations. First, this is not process, but a chain of events—pseudo-process; second, a universe in which "processes" admittedly occur is one thing, a universe conceived processually is quite another. Questions: Do non-literate cultures get beyond the level of pseudo-processes? In how far have any cultures other than the contemporary Occidental conceived of a universe as process—i.e. more than a universe in which processes occur? I think that this question is bound up with the distinction between infinity and indefiniteness; and I stand ready to be corrected if wrong in the belief that the Greeks conceived the latter rather than the former. But what about the flux of the Buddhist universe?

What was originally submitted above, couched in query form, was the proposition that the contemporary era of Occidental culture is characterized by an evolving interest in, and endeavor to seize adequately, pure process *per se*.

It is an extremely hard thing to do. What we habitually start with is a state; process is then conceived telically—the rationale that leads up to the state we are interested in. Growth is that process by which adulthood is achieved; the immature is by definition some kind of "uncompleted" organism. A legitimate definition, to be sure, from one standpoint; but not the only legitimate standpoint. Process is ancillary to state. However, in at least the field of mathematics we have already transcended this attitude, wherever process is conceptualized in its own right, without the necessity of postulating any state up to which it leads. Both Professor Northrop and Professor Bidney have indicated, independently, that Whitehead has occupied himself with the same problem, as a metaphysical one. (Beside the reference they both give, there is a briefer treatment in his *Science and the Modern World*.)

Quite obviously, the terms in my original communication are not those of

pure process; they, too, start with state and work back to process. Well, historically, our very ability to conceive process at all has started from the attempt to account for states. Such has been the course of events; whether there could have been another, I do not know. Psychologically, too, perhaps it is the only way it could have come about; but this is a point I am certainly not equipped to handle. Process so conceived still leaves the focus of interest upon the state it achieves; but it is the intermediate step toward the conceiving of process *per se*. It is my belief that, for Occidental culture at any rate, this last development is recent.

Certainly I am not the first to propose that process viewed as ancillary to state is an inversion of a reality. A state is, rather, a momentaneous abstraction from process. But we are not used, as yet, to thinking this way. But for lack of time and space, I believe it could be shown that in a variety of endeavors our culture is striving to conceptualize process *per se*. And if this be true, then it marks the first major reorientation of thought since the days when the Greeks invented philosophy. This would be no mere "development" in philosophy—any more than the Greek invention was—; it would be a major cultural event, of very rare caliber.

All of which is commentary upon the proposition that vocabulary is richer in expressions of state than of process. The question still is not, whether it is possible for us to deal with process via the devices of the English language; we are indeed doing it at this moment. The question still is that of the traditional thought-habits on which it has been constructed.

It is heartening that Professor Northrop and Professor Bidney recognize the validity of the observation in my original letter. It is significant that the condition is associated with Aristotle; but not because it originates with him; rather, because of the thought-climate he represents. For, as far as I understand the Indo-European languages, they are all of a piece in this matter; and it is hard for me to imagine how they could have been otherwise. They are richer in expressions of state than of process. So it is a very old heritage; one which the Greeks exploited but failed to transcend. All hail to them none the less: they achieved nobly enough by even conceiving the paradoxes of Zeno—let alone their inability to resolve them! The resolution had to wait upon the time of Galileian modes of thought which have transcended Aristotelean. And is it not Galileian thinking which has made possible our conceptualizing of process?

In spite of certain of the responding comments, therefore, the semantic and linguistic aspects cannot be so lightly dismissed. In so far as anthropology coincides with culture history, and culture history is at all concerned with *ethos*, the proposition originally submitted remains.

ECHOES: REACTIONS TO AMERICAN ANTHROPOLOGY

The object of this department is to help anthropology, and North American anthropology in particular, to see itself. In it we encourage anthropologists of all parts of the world to write about anthropology in North America, and scholars in all peripheral fields to relate anthropological methods and results to their own.—Editor.

THE JAPANESE CRITIQUE OF THE METHODOLOGY OF BENEDICT'S "CHRYSANTHEMUM AND THE SWORD"

Ruth Benedict's remarkable book on Japanese culture, prepared and written without a visit to the country concerned, was translated and published in Tokyo in 1948.* By 1952 the eighth edition was off the press, and interest continued to run at an extremely high pitch. This brief paper will not summarize the large number of standard book reviews given the volume by Japanese critics, but will concentrate upon a series of professional articles and scholarly symposia concerned with the book.

It should be understood that the translation of *The Chrysanthemum and the Sword* has appeared in Japan during a period of intense national self-examination—a period during which Japanese intellectuals and writers have been studying the sources and meaning of Japanese history and character, in one of their perennial attempts to determine the most desirable course of Japan's development. Much that Benedict said has been well known in Japanese social science and scholarship, but the organization of the materials was unique, the insights unusual, and the fact that the book had been written by an *American*, a *woman*, and one *who had never visited Japan* made it an object of vast and intriguing importance.

With the exception of two Americans who participated in one of the symposia, only one of the fifteen persons whose contributions are listed in the bibliography is a professional anthropologist. This single Japanese exception is Prof. Kunio Yanagida, the dean of Japanese ethnology and folklore, and a scholar of international repute. The rest of the group includes historians, legal scholars, sociologists, Orientalists, an economist, a social psychologist, and a philosopher. All these persons are regarded in Japan as leading authorities in their professions and fields. The near-omission of professional anthropologists from the group is a reflection of the fact that persons in Japan who are professionally classified as anthropologists are very few in number, and the majority of those are specialists in archeology and folklore. All but two of the scholars present a generally favorable attitude toward the book, although con-

* The translation was entitled *Kiku to Katana*, translated by Matsuji Hasegawa and published by *Shakai Shiso Kenkyu Kai Shuppanbu*. (Publication Section of the Research Institute for Social Thought.)

siderable reservation and criticism are apparent for all. The two wholly negative reports are those of Profs. Watsuji [A. (a) 4] and Tsuda [A. (b)], who feel that the book has grave shortcomings as a scientific work (Watsuji) and as a picture of Japanese culture (Tsuda). The more favorable critics not only stress the challenging methodological suggestions provided by the work, but also the signal contribution made by Benedict toward a more sophisticated understanding of the wellsprings of Japanese social behavior. Some also stress the potential value of the book in educating the Japanese people, who have tended to accept their own culture uncritically.

In the comments to follow, an attempt will be made to outline in brief the principal points made by these Japanese scholars with attention to methodology rather than to specific contentual aspects of the book.

Methodological Features Seen as Desirable

These are three: (1) the abundnace and depth of Benedict's data; (2) the evidence of productivity and efficiency in its utilization and organization; and (3) the strength and uniqueness of her conceptual approach.

(1) With regard to the first, Kawashima [A. (a): 264] states, "Japanese social scientists have tended to be speculative and little interested in using empirical data to check or support speculation". Approval is given to her use of *nisei* and *issei* Japanese as respondents, and it is pointed out that Japanese scholars have completely ignored Koreans and Chinese living in Japan as informants, while making studies of these cultures. This use of informants as a basic means for the collection of data is attributed by several commentators to the "positivistic" tradition of American social science, which should be emulated by Japanese scholars [Iizuka and Kawashima B. (a): 7; Kawashima A. (a): 2].

(2) Although efficient methods of handling data are now a commonplace in American social science—the property of both the unimaginative as well as the brilliant researcher—the Japanese scholars found in this efficiency and productivity something of considerable interest [B. (a): 6–9]. Such basic techniques and principles as the collective ownership of data in team projects; rational planning of research ventures, and a division of labor by specialty; systematic coding and filing of data; and the development of a common language and terminological system were all commented upon and recommended for adoption by Japanese scholars. Further development along these lines has been made in Japanese social science in the past three years. Cooperative field research has become *de rigeur*, and a theoretical and conceptual soul-searching is under way. Not all of this was due to the influence of Benedict's book, of course, but the book certainly symbolizes the trend and had no small role in effecting the recent acceleration. Several also took the occasion to plead for a reform in the method of writing Japanese—the involved and

excessively scholarly connotative system utilizing Chinese characters being thought to be too obscure for efficient, concise expression in the cultural sciences.

(3) The Japanese commentators are particularly attracted to—as well as critical of—Benedict's "theoretical" approach, which in this case means the battery of imaginative insights and concepts with which she interpreted her data. Particularly important here was her attempt to portray the *whole* of Japanese culture—a goal which has been common enough in certain branches of Japanese humanistic studies for many years. Kawashima states, "Benedict's primary interests and effort are devoted to the exploration of *reciprocal inter-relationships* among various Japanese modes of behavior and thought, and then to grasp the *total structure* (*zentai kozo*) which is constructed out of these interrelationships" [A. (a): 264]. Others specifically approve of the large role played by insight and creative imagination in her approach, and her avoidance of dull and mechanical statistical procedures.

In these features of Benedict's approach seen as desirable by the Japanese it is of interest to note that the first two—abundance and efficient handling of data—constitute the primary focus of attack on the patternist approach as developed by many American critics, who have felt that the patternist orientation has too little data and uses it unsystematically. Conversely, the configurationist-insight-intuitive methodological feature, also approved by the Japanese, constitutes the platform of attack on the empiricists—the data-gatherers and handlers—by the patternists. Thus the Japanese, cutting across the methodological and conceptual rivalries within the American orientations, see in Benedict's work all the most desirable features of American social science.

Methodological Criticism

Criticisms focus on the following six major issues: (1) the meaning of the term "Japanese"; (2) the meaning of the term "culture"; (3) problems in the use and interpretation of empirical data; (4) the need for an "institutional" approach to the analysis of the data; (5) the problem of historical change and its omission in Benedict's analysis; and (6) the need for an "interdisciplinary" approach to problems of whole-culture analysis.

(1) *The meaning of "Japanese"*. Benedict's concept of Japan and the Japanese was seen by the commentators to be in need of more precise definition. At several points in the book vague characterizations are given, which strike the Japanese as contradictory and different. Thus the scholars point out that in some places she is speaking of the "proverbial man in the street", in others, "all Japanese", in still others, "everyone", or "anybody". The lack of precise reference in these terms invited some commentators, especially Minami and Watsuji, to attempt to assign her specific patterns to real groups in the popula-

tion. Watsuji feels most of her patterns apply to the military and the fascist cliques during the last war [A. (a): 285–288].

In the opening paragraph of the book, it is stated that "conventions of war which Western nations had come to accept as facts of human nature obviously did not exist for the Japanese" [Benedict, *The Chrysanthemum and the Sword*, Boston, 1946, p. 1] This idea is developed in detail in the second chapter. In this chapter, the author contends that all aspects of Japanese behavior which violated the Western conventions of war became data by which to understand the Japanese worldviews and Japanese beliefs in human obligation relationships. The reader can easily replace all references to "the Japanese," with "one segment of the military," and when this is done, it is somewhat clearer that this book is about "patterns of Japanese soldiers" rather than "patterns of Japanese culture." But "patterns of Japanese soldiers" is still not sufficiently precise. It would be best if the book were entitled "patterns of the ultra-nationalistic group of Japanese soldiers" (p. 285).

Minami finds that her patterns are not assignable to any concrete group in the national society, but rather to "the ideal-typical Japanese—which is a logical construct" [A. (a): 272–273]. Minami then attempts to determine the relationships between "the ideal-typical Japanese" and "the real Japanese". He feels that the methods by which Benedict derived the ideal type from the real, and then used the type to interpret more of the real, are unstated and imprecise, though the results are imaginative and certainly challenging.

(2) *The meaning of "culture"*. Similar questions are raised with reference to certain ambiguities in Benedict's concept of culture. Most of the commentators accept her general definition of culture, or culture pattern, as high-level generalizations about the fundamental "design for living" of a society, but they do not understand how these constructs are related to the very evident institutional heterogeneity of Japanese society. Iizuka quotes the following passage from the book:

. . . Economic behavior, family arrangements, religious rites and political objectives therefore become geared into one another. Changes in one area may occur more rapidly than in others, and subject these other areas to great stress, but the stress itself arises from the need for consistency (p. 12).

The Japanese critics state that the above argument implies that "culture" is the ultimate determinant of change or the lack of it, and that it constitutes a kind of force or strain toward consistency. They feel [B. (a): 12–13] that this is an assertion that must not be assumed at the outset of an investigation, but must be empirically determined, and this Benedict has not done for Japan. As Yanagida remarks,

The theory of culture patterns . . . as I have inferred from the author's discussions in *The Chrysanthemum and the Sword*, seems to be concerned with those patterns which are permanently attached to a nation or a race, and which are not modified by changes in environment and by the power of time. This seems to me an important point of this

theory . . . whether it is a sad or a fortunate thing to have such permanent patterns is beside the point. But to determine whether or not such patterns are really empirically established, and how they can be established, is the demand of science [A. (a): 290].

(3) *Use of empirical data.* Being unclear about the implications of Benedict's concept of culture and pattern, the Japanese critics ask further questions about the ways she uses empirical data to establish the higher-level generalizations or pattern constructs. While they generally approve her abundance of data, they are critical about its use. They feel she is particularly guilty of ignoring meaningful temporal distinctions, *e.g.*, using materials of Tokugawa period currency to draw generalizations about present-day behavior. Minami points out [A. (a): 272] that she nowhere considers the fact that while her informants were Japanese who came to America during the Meiji or Taisho periods (from about 1900 to 1925), she uses their information to establish generalizations about the Japan of the Showa era (1926–present).

Some of the Japanese also criticize Benedict for failing to discriminate between differing institutional contexts of data. The most frequently-cited example here is her tendency to accept the propaganda formulations and rationalizations of the militarists as valid statements of Japanese "ideology," and read into them generalizations concerning the behavior and thoughtways of a whole people [Yanagida A. (a): 292–293; Watsuji A. (a): 285–287; Minami A. (a): 271]. As Miss Tsurumi points out,

> In *Patterns of Culture*, Benedict stated the reason why primitive societies were studied first by anthropologists was that configurations of culture in these societies were fairly simple, and that it would be difficult to conceive of such configurations of American civilization because there were different classes . . . . Yet she sees Japanese society as an uncivilized, undifferentiated, pre-modern society [B. (a): 18].

Others point out that Benedict's rejection of a statistical approach is justified on the basis of the peculiar demands of the study, but nevertheless they feel that awareness of statistical theory, even if quantitative data are absent, can lead the analyst to exercise caution and care in the use of any and all data and the making of generalizations. They feel that Benedict lacks completely a sense of the quantitative relevance of scientific statements [Minami A. (a): 272]. Several also feel that she fails to distinguish clearly between what people say they do and what they actually do—a failing that is typical of the patternist approach and may be traceable to the lack of concern with quantitative and other precise methodology [Watsuji A. (a): 285–287; Iizuka and Tsurumi B. (a): 14]. In one of the symposia, Miss Tsurumi comments as follows:

> There were people who objected to the idea of the East Asiatic Cooperative Sphere, and to the War. During the feudal period, farmers were obedient in public, but they freely criticized the authorities among themselves. The dual character of the Japanese

which was fostered by the repressive nature of feudalism is not well described in this book [B. (a): 14].

Finally, a criticism was made of her reliance on data concerning child-rearing for making pattern generalizations at the cultural level. The Japanese felt that the field of culture and personality is important and can contribute much, but the purely logical or plausible matching of the two cannot yield anything other than imaginative hypotheses [Minami A. (a): 273–274].

(4) *Need for an "institutional" approach.* As a conclusion to the criticisms just listed, most of the Japanese commentators recommend a series of detailed studies of behavior in differing institutional contexts, in place of, or at least to supplement, the whole-cultural approach. Kawashima's comments are typical:

> The author's methodology may be justified on some scientific grounds, but in spite of this, more emphasis should be placed upon the fact that Japanese society is in process of change and reform, and that there are many groups and strata in conflict with one another. . . . The patterns of Japanese culture as conceived by the author seem over-static and much too integrated. In order to carry on research where the author leaves off, it is necessary to analyze the differences and conflicts between various forms of behavior and thinking which arise from differences in social strata, region, occupation, age, and so forth [A. (a): 270].

(5) *The problem of historical change.* In addition to an institutional approach, the critics recommend one with greater attention to historical differences and change. This is stressed for the reason that Japan is a society which has repeatedly known great *changes* throughout its fabric, as well as certain *continuities* of tradition and social organization. Many features of Japanese values appear archaic to the outsider, and may indeed have a considerable historical depth, but they may be in actuality existing in form only, and quite changed in function. The culture may thus sometimes resemble, in formal aspect, a patchwork of elements of many different ages, and one cannot predict or understand the whole merely from a knowledge of one of the pieces. Such interpretations may result in a complete misreading—as Benedict tends to do in her acceptance of "feudal" aspects of Japanese culture as contemporary.

Watsuji states that Benedict might have been on safer ground if she had confined her study to "Japanese customs during the feudal period" or "the residue of feudal custom in contemporary Japan." For Watsuji, who in his boyhood saw his high schoolmates fascinated by the Japanese translation of *The Strenuous Life*, by Theodore Roosevelt, it is hard to understand Benedict when she says, "The Japanese, more than any other sovereign nation, have been conditioned to a world where the smallest details of conduct are mapped and status is assigned" (p. 170). Because of Benedict's tendency to

accept the "feudal" aspects of Japanese culture as contemporary, the critics state that she has failed to understand the dynamic aspect of contemporary Japan, and the ways in which the "old" and the "new" cultures have been in conflict [Kawashima, A. (a): 269–270; Tsuda A. (b); Kawashima *et al.* B. (a): 13–18].

However, one scholar in this group, Ariga [A. (a): 275–277] thinks that Benedict's approach may be justified for the reason that the structure of such a basic institution as the family has remained comparatively unaltered in present-day Japan. Ariga has advanced a similar thesis in his extensive studies of the rural family system.

(6) *Need for an interdisciplinary approach.* The substance of the Japanese criticism is that the study of a whole national culture like the Japanese requires a great number of small-scale researches on the different dimensions of social behavior, done by a variety of specialists and experts on differing institutional areas—not an intense single effort. The latter is applauded for its courage, insight, and pioneering spirit, but it does not substitute for a more empirical, and cross-disciplinary effort. Japanese scholars have picked up the "inter-disciplinary" notion with considerable eagerness and zeal in recent years, and are highly conscious of their own deficiencies in this regard. As Minami says,

> This book is instructive in the sense that it tells us that the social anthropologist is unable to analyze, single-handedly, the social behavior of a modern society. The possibility of achieving this can be realized only through the cooperation of neighboring social sciences [A. (a): 274].

Implications

Substantially, the criticisms just summarized are also those which might be made by most American sociologists and the non-patternist wing of cultural anthropology. Collectively, these Japanese appraisals of Benedict make up *the most thorough and exhaustive critique of a particular specimen of the whole-culture-patternist approach ever made*, or at least printed. It is a matter of regret that this critical exercise cannot be made available in full—inasmuch as anthropology has been lax in its internal serious (as contrasted to polemic) critical perspective.

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Murngin Kinship Re-examined, and Remarks on Some Generalizations

By A. P. ELKIN

I. THE MURNGIN "8TH LINE"

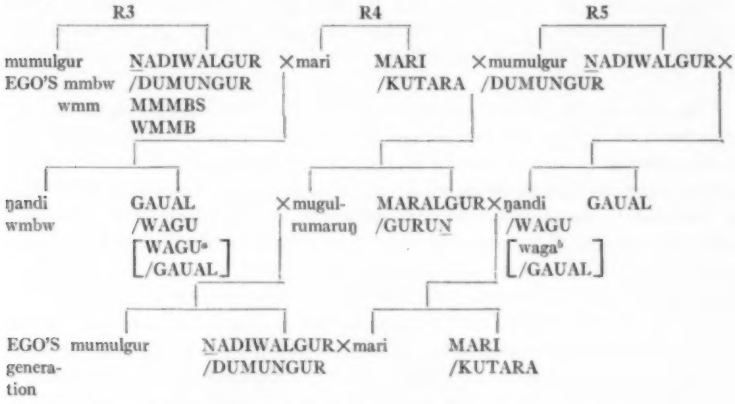
MR. LAWRENCE and Prof. Murdock (1949) have done good service in causing us to re-examine the Milingimbi (North-East Arnhem Land) social structure, particularly the kinship system. Quite apart from their problem of the number of patrilineal descent lines (Radcliffe-Brown 1951), it is important after a lapse of years, to restudy a society to see if any changes have occurred, and to determine the factors causing such changes. Perhaps too, any earlier errors and omissions can be made good.

During a tour in 1946 to determine future areas of research, I visited Milingimbi, the centre of Prof. W. L. Warner's research (1926–29), and also Yirkalla on the extreme east of the same "tribal" and cultural region. In Warner's day, the latter was little known. In the three following years I carried out short periods of research amongst the Rembaranga (Rainbarngo), Djinba and Ngalgbun of central Arnhem Land, whose marriage norm, subsection system and moieties are the same as amongst the Murngin, or Wulamba, as the whole group of clans now call themselves. Towards the end of 1946, R. M. and Catherine Berndt, who had just finished over two years' field-work in the north-west and north of the Northern Territory, went to Yirkalla for almost a year. They also spent two short periods at Milingimbi. Further, in 1949, I took Lawrence and Murdock's article into the field to look for their "8th line" amongst North-East Arnhem Landers whom I would, and did, meet in Central Arnhem Land and at Darwin.

Looking for the Eighth Line

The search was easy. There is no confusion, either in genealogy-recording or in discussion with regard either to MMB (the "granny"), his wife, or the latter's brother. The last in this region is *NADIWALGUR*. The question raised was, "whom did he and the males of this patrilineal descent line marry?" for Lawrence and Murdock (pp. 64–65) were not satisfied with Prof. Warner's statement (pp. 115–116) that these men married women called by terms of the preceding line but one. The answer was *mari, mugul rumaruy mari* and so on, the sisters of *MARI, MARALGUR, MARI* respectively. This confirmed Prof. Warner's statement, and disagreed with Mr. Webb's report (See Lawrence & Murdock 1949) that the MB (*GAUAL* or *GALWAL*) of *NADIWALGUR* is EGO's *GORON* (*GURUN*) from the fourth collateral on the father's side. This line is not shown on Prof. Warner's chart, but was not unknown to him. The said MB is *MARALGUR*, the brother of *mogul rumaruy*. Thus, starting from Warner's extreme right hand column (R3), and extending out to

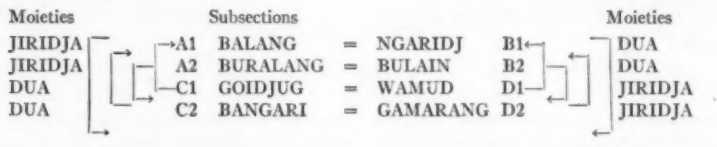
R4 (the missing (?) eighth line), my field chart shows from EGO's generation upwards: (Reciprocal terms follow the stroke /; R3 indicates, in the usual way, the third collateral on the right, or mother's side, and so on, just as L1, L2 and so on would indicate the collaterals in sequence on the left, or father's side.)



Mr. Webb, in spite of his knowledge of the people and the language, was misled possibly by the reciprocals of the terms, but mainly by the allowed alternative marriages. For example, my informants made it clear that when the above *GAUAL* (WMMBS) dies, a man I call *WAGU* (not my own *WAGU*, SrS), marries his widow. This could give the impression that the son and father of *NADIWALGUR* were *WAGU* (See *a* on Table). Similarly, my genealogies show that *MARALGUR* in R4, who calls EGO *GURUN*, marries *ŋandi* ("mother")—the sister of a *GAUAL* and daughter of another *NADIWALGUR* (R5). Another wife, however, may be (is in a genealogy) *wagu* (reciprocal *GAUAL* (See *b* on Table). This is also apt to give the impression that the descent line is *NADIWALGUR-WAGU*, unless careful genealogies are recorded. Mr. Webb wrote "I was fortunate enough to discover a group of living persons well known to me whereby I ascertained that the father and son of *ngatiwalkur* are *waku*" (Lawrence and Murdock: 64). My correspondence with Mr. Webb in 1939 shows that I tried to convince him that Prof. Warner was correct in giving *GAUAL* for the son of *NADIWALGUR*.

The genealogies show that in this latter alternative and additional marriage, the subsections of the two wives are different, though belonging to the same moiety. This is important, because of the constancy of the matrilineal cycle of subsections. Indeed, I do not agree with Prof. Radcliffe-Brown's statement that the Murngin subsections are of no real importance (presumably

in the social structure). Though recent in Prof. Warner's day, they were already influencing the kinship structure. Omitting the feminine terms for subsections, the system is:



The symbols A1, A2 and so on, in the left hand cycle are arranged differently from Prof. Warner's scheme (p. 119), adopted by Prof. Radcliffe-Brown (p. 50). The former did not record the definite preference for marriage between certain subsections, e.g. Balang-Ngaridj, Buralang-Bulain, Goidjug-Wamud, and Bangari-Gamarang. It was not, and is not, a simple matter of Buralang marrying Ngaridj or Bulain, and of Balang doing the same, and so on. The subsection preference is an expression of the kinship preference, namely for marriage with mbd. Granted that this is followed out, the mbd of BALANG is Ngaridj, whereas Bulain is a srsd, the daughter of a *WAGU*, an alternative marriage which is in favour today. The arrangement of the subsections is as Mr. Webb reported to me in 1933, and which Mr. and Mrs. Berndt and I have independently checked in genealogies in the field (Elkin 1933; 1950: 8–12).

This difference in arrangement of the subsections for the purpose of marriage, however, does not affect the matrilineal cycle of the actual named subsections. Therefore, A1, BALANG'S sister's son, *WAGU*, is C2, BANGARI, whether that sister is mbd to a NGARIDJ husband or srsd to a BULAIN husband. Further, BALANG's mother's brother, *GAUAL*, must be in C1, GOIDJUG; likewise his MMB *MARI*, must be in A2, BURALANG. Thus *GAUAL* and *WAGU* are not equivalent, and this fact is indicated not only by their different kinship terms, but also by their being in separate subsections. Nor is there any way in which *WAGU* can be self-reciprocal. My *WAGU*, close or distant, calls me *GAUAL*, and the more *WAGU* I have, the better I am pleased. Lawrence and Murdock (p. 60) say that in line L3, their line 7, *Wagu* is self-reciprocal.

Further, if I am BALANG, my MMMBS or WMMB, my *NADIWALGUR*, is normally B2, BULAIN, whose children, if he marries into his mbd subsection A2, buralang, are in C1, Goidjug, the subsection of my own MB (*GAUAL*). On the other hand, if he marries "alternatively" into his sr sd subsection, A1, Balang, his children will be in C2, Bangari, to which my *WAGU*, SrS, belongs. Thus while the son of my *NADIWALGUR*, of the second generation is normally my *GAUAL*, he may be a *WAGU* to me. In the latter case, son's children of *NADIWALGUR* can be my *galei*, wife, and

wife's brother. Further, the son of this male GALEI is *WAGU* to me. Thus, we have a case of both father and son of *NADIWALGUR* being *WAGU* instead of *GAUAL*. I suggest that it was an alternative case of this type which Mr. Webb reported to Lawrence and Murdock. A good genealogy of that case with reciprocal terms and subsections would have cleared the matter. But even if this sequence were the norm, it would *not* be the equivalent of patri-lineal descent line L3 (Lawrence and Murdock's line 7), for *dumungur* is the reciprocal of *NADIWALGUR*, not the same person. And if it were, it would reduce the system to six "patri-lines," which seems to be what Lawrence and Murdock have done. Moreover, the statement that "in this usage" *WAGU* is self-reciprocal, is strange. Two persons EGO calls by one term are "brothers" to each other, or "son's son." Similarly, with Lawrence and Murdock's new eighth line, in which *guruy* is said to be self-reciprocal. Apparently, two persons actually related as a man and his son's son or as a man and his mother's brother's wife's brother's son (all of whom EGO calls *GURUN*) are thought to call each other by that term also. This is not the case.

Finally, these two writers give as the eighth line, one which repeats their sixth, the second collateral on the father's side, *GUDARA-GURUN*, for which type of thing they seem to criticize Prof. Warner. But (pp. 59–60) they make a further error in regarding *NADIWALGUR* as marrying *gudara*. It is EGO's *DUMUNGUR* who does this. The men of their LINE 1, *NADIWALGUR*, *et cetera*, marry into their Line 8, the new *GUDARA-GURUN* line. They are, however, right in saying (p. 65) that there can be no "final limits" to indefinite lateral expansion. But all that Prof. Warner could mean is the fact that outside relations are "thrown back" *in terminology* into the lines of those relations who are of close significance to him. Therefore, they err in saying that his extra line (*MARI-MARALGUR*) our R4, into which the *NADIWALGUR-GAUAL* line marries, would require through reciprocal terminology, a "ninth line," our L4, whose men would marry into Lawrence and Murdock's patri-line 7, our L3. The reciprocity is between EGO and the *MARI*, *MARALGUR* and others of R4, not between them and members of a descent line on EGO's father's side, L4 or any other. It is only theoretical patterning which leads into L4. *MARI* of the grandparents' generation in R4 calls his Sr H Sr DDH (i.e. EGO), *GUDARA*. Therefore, EGO applies that term to his own Sr H SrDDH who would be in L4 on his grandchildren's generation level. But his interest in this man or the latter's F and FF and so on, is hypothetical.

Therefore, I agree with Prof. Radcliffe-Brown's theoretical criticism of the "eighth" line, and reiterate my appreciation of the work done by Prof. Warner.

In the meantime it became clear that more detailed study of the social structure and kinship at Milingimbi was required, particularly with regard to alternative marriages. Therefore, as Mr. and Mrs. Berndt were working during

the wet season of 1949-50 at Oenpelli, in western Arnhem Land, I arranged for them to *return* to Milingimbi to concentrate for three weeks on kinship and related aspects of the structures. The region is important because there has been little, if any, break-down of the social structure or ceremonial life. This is even truer at the Yirkalla side where these two very experienced field-workers, as explained, had already got a grip on the social structure, culture and language. Thus, among the three of us, we have more facts, and we can be sure of them, as Prof. Radcliffe-Brown (1951: 54) suggests might be necessary, with reference to Mr. Webb's statement on the 8th line. This material, which confirmed the above conclusion, will be discussed in *Oceania*.

II. REMARKS ON SOME GENERALIZATIONS

Reciprocals. The theorist, however, must be careful of all generalizations, minor or major. Thus, Prof. Radcliffe-Brown (1951: 46) says that column L1 contains the persons related to EGO by the relationships which are reciprocal to those in which EGO stands to persons in R1, and likewise with L2 and R2, and so on. He was using Prof. Warner's Chart II. But *NATI* (*NADI*), EGO's MF in R1, is not the reciprocal of *DUE* (*DUWEI*), EGO'S FF SrH in L1. *NATI* is the reciprocal of *KAMINVER* (*GOMINJAR*), and the latter is the term for FF SrH, thus justifying Radcliffe-Brown. But we cannot alter the *KAMINVER*, EGO's Sr SS in L1, which is *not* the reciprocal of *GALLE* (*GALEI*), EGO'S MBSSS in R1. Still, in some alternate marriages, the former is *DUE*. The alternate and allowed marriages make such generalizations difficult.

Identification in Kinship

On a wider canvas, I am chary of Prof. Radcliffe-Brown's use of the word identification in connection with certain uses of kinship terms, especially the classification together of persons in alternate generation levels. Thus, "the identification of a man with his father's father" (Radcliffe-Brown 1951: 47–48) is not true in actual life, even if they do call each other by the same term. This latter connotes the relationship and behaviour pattern between persons who use the term reciprocally. The behaviour between them is not *identical*. It *is* reciprocal. This is made quite clear when the reciprocal relationship is expressed by a pair of terms, e.g. elder and younger brother as in the Northern Kimberley and in the Yir-Yoront (Cape York Peninsula), or by special terms, such as *MARIKMO-MARAIDJA* in the Murngin. The basic significance of classifying in the latter FFSrS as *WAKU* ("SrS") lies I think, in the prohibition on marriage with fsr(hd)d, that is, with ffsrsd; for EGO does not marry the daughter of his "own" or primary *WAKU* (SrS), who may be his own DH. In this system, EGO marries the daughter of his *GALWAL* (*GAUAL*), MB. This prohibition is expressed in the same way in the Karadjeri

(inland, near La Grange, Western Australia) and Yir-Yoront systems. On the other hand, where bilateral cross-cousin marriage is allowed, as in the Kariera (Western Australia), Yeidji (Northern Kimberley) and Wailpi (South Australia), FFSrS is classified with MB, who is also WF. In many tribes, too, which prohibit cross-cousin marriage, FFSrS is also classified with MB, the MB being still the type of WF, but another term is suffixed to the term for such "secondary" MB, or else substituted for it, if he is, or is regarded as, WF. The Dieri (South Australia), however, act differently, and use the term for SrS (*TIDNARA*) to denote WF, and apply it to FFSrS to signify WF. Thus, generalization on a wide scale is not straight-forward.

Universality of Patrilineal Local Groups. Field-work in widely separated parts of Australia since 1927, together with continual examination of field-work reports, has made me hesitant to accept some generalizations which used to appear obvious and substantiated. Prof. Radcliffe-Brown sums up two of these, which are closely related. "Everywhere in Australia the fundamental basis of social organization is a system of patrilineal local groups or clans of small size"; and again: "the social organization of Australian tribes is always based on patrilineal descent" (pp. 38, 40).

These are true in many parts of the continent, including the De Grey River and neighbouring districts of Western Australia, in which Prof. Radcliffe-Brown did his main Australian field-work. But it is definitely not true of Western Arnhem Land and of Melville and Bathurst Islands, which not only possess matrilineal phratries, and in the former case, also matrilineal moieties and subsection cycles, but are also matrilocal. In Western Arnhem Land, however, there is a patrilineal spiritual tie to the country: that is, the spirits of a patrilineal descent group have been mediated from the "Dreaming" through a particular Dreaming place. But this does not entail ownership of tracts of country with hunting rights. Nor am I satisfied that the social organization of south-eastern Queensland and north-central New South Wales was based on patrilineal local groups and patrilineal descent. The tribes there were predominantly matrilineal, membership of clans, moieties and sections (indirectly) being so determined. I have known some of the old men well, but I could not glean from them that marriage was necessarily patrilocal, or that hunting and food-gathering areas were divided amongst patrilineal groups, natural as this might seem to us. Further, tribes in the Victoria River region (north-west central Australia) place chief emphasis on their matrilineal clans, and these mix together for social and economic purposes. Years ago Dr. W. E. H. Stanner, writing to me from the field, said he could not get the natives there to understand that they should have patrilineal food-gathering clans (hordes) and countries. Likewise in the Fitzmaurice and Daly River districts, patrilineal clan-spirit-centres are recognized, but not patrilineal, patrilocal hunting countries for the clans or other small groups (Elkin 1950: 1–20).

Thus, once again, generalization is difficult. In the arid regions of Central and Western Australia, Radcliffe-Brown's generalizations hold good. They are also true for certain coastal areas where definite matrilineal forms of social structure have not spread, or have not been integrated with the rest of the structure, as in the Northern Kimberley, North-East Arhem Land, the lower Murray River, the coast of New South Wales, and much of Victoria.

In most *good* food-gathering countries, such distinctions, if they did exist, have become blurred, for both tribes and clans spend much time together in social, economic and ceremonial activities. The distinction between tribes becomes vague, as does the distinction between matrilocal and patrilocal marriage. If matrilineal social structure predominates, the patrilineal nature of the local hunting group becomes less clear-cut, and patriliny as an actual institution hardly exists, except for the tribe as a whole, unless there are patrilineal "cult-lodges" with their ties to local sanctuaries, as in north-eastern South Australia. The determination in the field, of the "countries," as distinct from the spirit-centres and cult-centres, of local groups in most good food-gathering regions, when tribal life is vigorous, is so difficult, that one may well wonder whether these are always significant sub-divisions of tribal territory.

Moieties. Finally, I would urge Prof. Radcliffe-Brown to reconsider his decision (p. 39) to apply the term endogamous moiety to the grouping together of alternate generation (genealogical) levels as occurs in what I have called the Aluridja group of tribes of western South Australia and the neighbouring region of Western Australia. Dr. Roheim, who had worked amongst an Aluridja group in 1929, was the first, as far as I know, to call these named alternate generation levels, endogamous moieties, Dr. Bateson (p. 269) referred to the same feature of social structure amongst the Iatmul, as "lines" or "ropes."

True, the division of a society into two groups for some purpose (in this case, social and ceremonial), constitutes a "half-division." But I think it preferable to retain the practice of using the term moieties for the division of society into two *descent* groups, usually named, of ceremonial and social significance. The two alternate generation levels, forming two endogamous "lines," are not descent groups. Moreover, the terms denoting them are reciprocal. Members of their own "line" call each other, if Kukata tribe, *Nganandaga;* likewise the members of the other "line" (their parents' and children's levels) use the same term with reference to each other. But members of one "line" call those of the other, *Tanamildjan* (Elkin 1939–1940: 212–214). Actually, these are behaviour terms and not the names of groups. One of the main rules they embody is that marriage should not occur between persons of succeeding generations. The "right" person belongs to one's own generation level or to the level two above or two below.

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Géza Róheim 1891-1953

By GEORGE DEVEREUX

GÉZA RÓHEIM was born on September 12, 1891 in Budapest, Hungary, and died in New York City on June 7, 1953, shortly after the demise of his beloved wife Ilona. He became interested in folklore while still in the *gymnasium*, and in psychoanalysis while attending the Universities of Leipzig and Berlin. He obtained his Ph.D. (in geography) from the University of Budapest in 1914 and then joined the staff of the Hungarian National Museum. He was psychoanalyzed in 1915–16 and engaged thereafter in the practice of psychoanalysis. In 1921 he was awarded the Freud Prize. From 1928 to 1931 he did field work among the Somali, in Central Australia, in Duau (Normanby Island) and among the Yuma. In 1938 he joined the staff of Worcester State Hospital as a teaching associate, remaining there until 1939, when he entered private practice in New York. In 1947 he made a field trip to the Navaho.

Róheim was a tremendously productive and creative writer. His publications include more than 15 books and monographs, some 160 papers and numerous book reviews, and he edited the annual: *Psychoanalysis and the Social Sciences*. Even so he left numerous unpublished manuscripts and field notes. He was a Fellow of the American Anthropological Association, a member of the council of the American Folklore Society (1944–46), a corresponding member of the Hungarian Ethnographic Society and an Honorary Member of the New York Psychoanalytic Society.

Although, like many pioneers, Róheim received relatively little formal recognition from official science in his lifetime, his friends and disciples celebrated his sixtieth birthday by the publication of a Festschrift: Psychoanalysis and Culture.[1]

Róheim's true stature, as a creator of the first rank in the field of the psychoanalytic study of society and culture and in the field of culture and personality problems, is just beginning to be recognized. Under the guest editorship of the present writer a special issue of the *American Anthropologist* is being prepared for publication next year. This issue will contain a basic appraisal of Róheim's work and a comprehensive bibliography of Róheim's writings.

Time will prove that, like his beloved Horace, Róheim too could have exclaimed: "*Exegi monumentum aere perennius.*"

[1] Muensterberger, W. and G. B. Wilbur (eds.): Psychoanalysis and Culture: Essays in Honor of Géza Róheim. International Universities Press, New York, 1951.

Book Reviews

General and Theoretical

Cultural Sciences: Their Origin and Development. Florian Znaniecki. (viii, 438 pp., $6.00. University of Illinois Press, Urbana, 1952.)

This book is neither quite a statement of a systematic theoretical position nor yet a history of the social sciences. In part, it is both of these and also a philosophy of social science. Perhaps the most accurate characterization would be that of a treatise which Znaniecki has written as the capstone of his career, weaving together out of his many previous researches and prodigious reading the theoretical points and empirical generalizations that now seem to him most important. There is much in the product which is genuinely integrating, thoughtful as well as learned; the reviewer, however, was often unfavorably reminded—in spite of the European scholarship—of American textbooks in sociology.

The cultural sciences, for Znaniecki, embrace what today are usually called the social sciences plus the humanities. Actually the substance of the book deals overwhelmingly with philosophy, history, and sociology which is regarded as "the basic cultural science, just as physics is the basic natural science" (p. 11). The Introduction and the first three chapters deal instructively with various conceptions of order and their historical development. Chapter 4 suggests that a human-centered metaphysics, accepted by scientists, "has been the chief medium through which metaphysical controversies have interfered with the development of sciences, especially cultural sciences." The two following chapters are essentially epistemological. Znaniecki well says " . . . the task of the scientist is not to define the true essence of the data he investigates, only to observe the factual relationships among them and to discover whatever order exists in these relationships" (p. 151). The author next develops his theory of action and his present formulation of the famous concepts of "attitude" and "definition of the situation" introduced long ago by W. I. Thomas and himself in *The Polish Peasant.* He speaks eloquently of values as an essential component of human action. His central conception is that of "the humanistic coefficient": " . . . every situation must be studied by the investigator as it is experienced by the particular human individual who is conscious of it" (p. 242). " . . . comparative investigation of ideational attitudes is an indispensible task for all students of culture, for it opens the only road to scientific generalizations about human actions" (p. 260). Sociology is defined as "the science of order among social actions" (p. 387). The book closes with an exceedingly optimistic statement on "practical applications of cultural sciences":

> . . . I like to imagine that sooner or later the solution of all important human problems will be entrusted to cultural scientists, and that sociologists will assume the task of ascertaining how innovations of specialists in various realms of culture (including the realms of natural sciences and of techniques) can be cooperatively used by social groups of practical agents for the best advantage of humanity. This does not mean that the future of mankind would be planned and controlled by sociologists, as Comte imagined, or that the human world would become not only socially united, but culturally uniform. It means rather that sociologists would function as intellectual leaders in the ceaseless course of differentiation and integration of social roles and social groups throughout the world. . . .

There are nice points of theory and delightful bits of information scattered through-

out this book, like plums in a pudding. But, in the reviewer's opinion, *Cultural Sciences* is not, as a totality, profoundly original or creative. And, in spite of the author's immense learning, there are some curious omissions of recent works directly relevant to Znaniecki's central themes—e.g. Kroeber's *Configurations of Culture Growth.* Indeed, in a review in an anthropological journal, it should be specifically remarked that Znaniecki makes only sparse and casual reference to the work of modern American and British anthropologists. Boas appears once (in a footnote); the writings of Kroeber and Sapir are nowhere cited.

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Toward a General Theory of Action. TALCOTT PARSONS and EDWARD A. SHILS, Editors. (xi, 506 pp., $7.50, Harvard University Press, Cambridge, 1951.)

Parsons and Shils have produced an important failure which must be considered carefully by everyone interested in developing a basic body of social science theory. It is perhaps unfair to class the book as a failure, for the editors are explicit in claiming only exploratory value for the work and in pointing out the need for testing and revision. Nevertheless, not only is the theoretical structure far removed from the level of theory which generates most significant research hypotheses but, especially for anthropologists, the faults are sufficiently serious that few will be stimulated to put the theories to the test of research.

The work is the joint product of four psychologists, three sociologists, and two anthropologists. This division is clearly reflected in the orientation of the general theory. It is of significant interest that the anthropologists register the only major caveats entered by the collaborators. The most important of these is contained in "Some Observations on Theory in the Social Sciences" by Richard C. Sheldon, who indicates not only that the theory as it stands is incomplete, but that it is essentially an abstract system of categories which must be given factual meaning in terms of operations and from which must be derived relationships subject to empirical test.

The theory places human action in three major categories: personality, social system, and culture. Most of culture, as anthropologists use the term, is placed in personality and social system, leaving for culture only systems of ideas and beliefs, systems of expressional symbols, and systems of value orientations. Reference is also made to skills and, on occasion, to objects or artifacts as products of action, things which the authors pass over hastily, since they obviously do not know what to do with them. So long as such things as social structure and other learned behavior patterns are adequately dealt with in a general theory, it perhaps is not too important what the categories are labeled. But the proposed organization of categories certainly requires that every definition of culture from Klemm to the present be discarded and a radically new definition be developed.

The general theory is individually oriented and, by implication at least, is strongly reductionist, leaving little or no possibility of dealing with social and cultural phenomena in their own terms. The authors admit strong Freudian influence but seem unaware of a major dilemma into which they are led by acceptance of the view that there is a fundamental conflict between the individual and his society and culture.

In the theory, culture and society (and all action) are seen as responses to the needs of the actor. If this be true, then for the normal individual conflict should be relatively non-existent. The theory recognizes, however, that culture and society create some types of needs. But if it be argued that it is in this area that conflict arises, such conflicts may not be viewed in their entirety as arising out of intrinsic qualities of the individual, but at least in considerable part as arising out of the inadequacies of the sociocultural systems which do not adequately prepare their members in the socialization process for the demands made upon them in adult life. Clyde Kluckhohn indeed walks up to the very brink of this problem when he gives a long and pertinent quotation from Dorothy Lee (p. 425 et. seq.), but he then hastily backs away from it.

Another weakness which may be mentioned relates to the selection of alternatives in action. It is here that values and value orientations are seen as basic, but it seemed to this reviewer that there was confusion in the handling of alternatives as seen by the observer and as seen by the actor. More seriously, there seems inadequate recognition of the part played by culture in determining the perception of alternatives, both for the actor and the observer. Particularly in the case of the observer it is assumed that all alternatives will be recognizable, a view that the history of science amply confutes.

The book is divided into four major sections. The first gives a general statement of the theory as agreed upon substantially by all the collaborators (although Clyde Kluckhohn enters a statement of doubts and Sheldon gives a general critique already mentioned). In Part II Parsons and Shils spell out of the categories and sub-categories involved, in detail. In Part III Edward C. Tolman gives a psychological model which I am incompetent to evaluate. Part IV contains an uneven collection of papers purportedly dealing with the application of the theory. Gordon Allport writes on various approaches to the study of prejudice, Henry A. Murray on a classification of interaction, Robert Sears on social behavior and personality development, while Samuel A. Stouffer offers a study of technical problems in analysis of role obligations. The last two are reprints, and none are directly related to the general theory except insofar as they refer to categories used in it. Perhaps the most interesting paper for anthropologists is Clyde Kluckhohn's exploration of the problem of definition and classification of values and value-orientations, which represents the most extended and systematic attempt thus far to deal with values in anthropological terms.

To do justice to *Toward a General Theory of Action* would require a lengthy review article rather than this relatively brief comment. Consequently, in conclusion, it should be reiterated that this is an important book in the development of social science theory.

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The Tree of Human History. Alan Houghton Broderick. (253 pp., 5 maps, $4.75. Philosophical Library, New York, 1952.)

In this period of increasing specialization, it is refreshing to encounter an author who has both the knowledge and the will to attempt a history of culture on a global basis. In preparing this work he has made use of the most up-to-date material available. Carbon 14 dates are given for all regions where they have been established. His

bibliographies include numerous titles from 1950 and 1951, while the bulk of the authorities cited are post 1930. Even finds as recent as the Fontechévade skulls, the latest African proto-hominoids and the Angle-sur L'Anglin frieze find their place. Lastly, the author is to be complimented on a truly remarkable piece of condensation. He has compressed most of human pre-history into barely two hundred and fifty pages and presented it in an easy, interesting style.

In a work of such scope a certain number of errors of both omission and commission are inevitable. Thus one may doubt whether any history of culture should completely ignore Southeast Asia, Oceania and Australia, or limit the discussion of Japan to a mere four and a half pages appended to the chapter on China. The whole discussion of Africa is included in the single chapter "Egypt," and contains no mention of the high protohistoric cultures of West Africa and the Sudan. North and South America are treated as a unit with the Maya as the center of interest.

The work also reveals that ignorance of living "primitive" cultures so frequent among archeologists. Thus the author states that: "painted pots need for their firing quite elaborate kilns" (p. 130), in spite of the Pueblo evidence. He ignores the importance of wild vegetable foods in most "primitive" economies and says: "It is possible that the first crops were used to attract or retain wild or semi-wild animals" (p. 107). He rather consistently underrates the possibilities of diffusion and denies trans-Pacific influence on the higher American cultures with a vigor few Americanists would manifest today.

However, these criticisms are of a very minor sort. The author is to be complimented on his success in an excessively difficult task.

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ETHNOLOGY AND ETHNOGRAPHY

Cayuá Culture Change: A Study in Acculturation and Methodology. JAMES B. WATSON. (144 pp., 20 illus., 1 map, $2.50. *American Anthropologist* Memoir Number 73, April, 1952.)

A perusal of the not inconsiderable literature in the field of acculturation based generally on a high quality of field work reveals relatively little attention paid to conceptual considerations or problem stating. The work in review makes a contribution toward this end. By explicitly delineating his field of inquiry to a group experiencing European culture contacts for close to four centuries but *intensively* for only the past *fifty* years, Watson has made his conclusions amenable to comparison with the effects of this process among other similarly affected groups.

The Cayuá are a people of Tupi-Guarani linguistic stock in southernmost Brazil near the Paraguay border. Etymologically the term Cayuá seems to mean "forest" or isolated people, as compared with their Guarani cousins who were early missionized or reduced to a state of peonage. Because they were until recently on the fringes of European penetration, little has been recorded of the first two centuries and more of Cayuá cultural relations with settlers in their region. In view of the inadequacy of the post-contact historical materials the author was led to develop a conceptual model for the analysis of situations of this sort. This he speaks of as a "configurational" or "struc-

tural" approach, as opposed to a historical chronicling of events and their consequences. It consists in the isolation of certain significant changes readily discernible between past and present Cayuá culture in terms of, first, identifying factors of cause and effect and, second, establishing relationships among causes and effects. "Causes of acculturation mean those changes in the total Cayuá environment initially traceable to the presence or influence of an alien European society, and which have produced changes in Cayuá culture" (p. 128). As a second step effects are distinguished between *primary* or immediate and *secondary* or derived. The latter distinction is an important one inasmuch as it explicitly seeks to clarify the relationship between initial direct consequences of contact events (primary) and subsequent implications for group adjustments. Among the Cayuá for example, the shift in agricultural labor from female to male pre-eminence appeared to follow *indirectly* from (1) the initial introduction of new tools (the axe and the machete) which they observed were used only by white *men* and (2) working out of Cayuá men on white lands, principally as farmhands.

Another useful conceptual distinction consists of that between *initiating* and *consolidating* factors of change (p. 95). Thus the introduction of metal tools, *per se*, did not automatically involve the exchange of agricultural surplus with whites. It did eventuate in an increase in output, however, which made possible such exchange, and hence strengthened (or consolidated) the tendency toward the goal of increased output. Consolidation at the socio-cultural level, therefore, parallels the process of reinforcement of new learning at the social-psychological level.

Watson has also indicated the inadequacy of the "trait" concept for assessing the nature of acculturational processes. Culture contact, that is, involves not only the impact of cultural products and elements of behavior, but also the creating of adjusting conditions. The Cayuá, for example, experienced a restriction in size of territory and spatial mobility with important consequences for adaptation to new circumstances (pp. 12–13).

On the whole this study adheres admirably to its purpose, and makes a contribution to the analyses of dynamic processes where historical information is limited, within the limits imposed by the author. (He is concerned with *social-cultural* processes, and not with social-psychological factors of change.) It provides also a useful addition to Cayuá ethnography which the writer intends to amplify in a separate monograph.

The sections on Cayuá economy past and present (pp. 24–28 and 63–82) in the reviewer's opinion would be likely to raise eyebrows among economists for including descriptions of the technological systems. This does not detract from its conceptual and methodological implications for a growing systematics of acculturation theory.

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Tlaxcala in the Sixteenth Century. CHARLES GIBSON. (xvi, 300 pp., $6.00. Yale University Press, New Haven, 1952.)

This is an important contribution to the understanding of culture contact in central Mexico during the eighty years following the Conquest. It is a careful synthesis of data from the documentary sources for Tlaxcala by an historian interested in problems of acculturation and concerned to determine the nature of Indian resistance to, ac-

ceptance and integration of elements of Spanish culture. While the body of the book is occupied with the establishment of the rather meager documented facts bearing on these matters, the summaries at the end of each chapter and the general conclusions give interpretations of the facts which shed much light on the processes of culture change among the Tlaxcalans between 1519 and 1600. The book constitutes a more critical and penetrating analysis of Tlaxcalan acculturation than has hitherto been available.

Gibson deals successively with Tlaxcalan policy toward and reaction to Cortes's campaign, the eighty years of missionary activity following the Conquest, the imposition of Spanish government and the initial integration of native and Spanish organization, the disintegration of Indian political institutions, the outlines of Tlaxcalan social and economic life, the pressures of Spanish tribute exaction and the Tlaxcalan reaction to these, including emigration and establishment of colonies during the later sixteenth century. There is also an interesting summary of the movements of Tlaxcalans in the two subsequent centuries to the northern borders of New Spain as far as Santa Fe, New Mexico. The conclusion is an excellent and all-too-brief interpretation of cultural processes in which the author discusses Tlaxcalan history as "an instance of 'primitive' adaptation to new conditions."

The view of Tlaxcalan response to contact that emerges is a little different from that which has been accepted. The efforts of Tlaxcalans to maintain their own religion, political and social organization are documented. There was not an immediate cultural disintegration. Rather the native class structure and political organization was integrated with the imposed Spanish system. This process of integration continued for half a century after the Conquest. It was the late sixteenth century Spanish colonization with resulting encroachment on Indian lands, harassment of native agriculture, decimation of population by periodic epidemics, and the exacting of a fixed burden of tribute despite changing economic conditions which broke Tlaxcalan culture and ultimately disrupted orderly processes of cultural fusion.

Gibson's book, the 56th in the Yale Historical Publications Miscellany, is an extremely useful case study for piecing together the picture of the Hispanization of the Nahuatl-speaking people of the central Mexican plateau. If it is a little disappointing in the small amount of detail about Tlaxcalan culture, that is the fault of the data. The author has made clear what there is to work with. The seven lengthy appendices listing, among other things, the known facts about the pre-conquest rulers of Tlaxcala, the cabecera successions, and the details of the royal privileges granted to Tlaxcala may yield, with a little further analysis, a somewhat fuller picture of the nature of Tlaxcalan culture.

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Religion and Society among the Coorgs of South India. M. N. Srinivas. (Foreword by A. R. Radcliffe-Brown. xvi, 267 pp., 12 pls., 4 maps, glossary, appendix. £6.00. Oxford University Press, London, 1952.)

This is the first full account of the social structure and ceremonies of a south Indian peasant people, written by the occupant of the chair of Sociology at the new

M.S. University of Baroda. The account is excellently rendered. Although field materials were gathered more than ten years ago, guided simply by canons of sound ethnography and the author's extraordinary flair for ritual details, they have been completely reworked by him under the inspiration of Oxonian functional theory. The product is not only an admirable field document and an exemplification of functional analysis, but also a book of fresh insight into Hindu life. With its neat ordering of many complex matters and its superb index, it cannot but become a guide and criterion of future comparative work in India.

Coorgs are the dominant caste of a single district, isolated high in the mountains between Mysore and Malabar. They consume liquor and most kinds of meat, arrange the remarriage of their widowed daughters, recognize Brahmanical gods only through vague correspondences with their own local godlings, and make next to no use of Brahmans in their domestic ceremonies. Nevertheless, Coorgs stand second only to Brahmans in the local caste hierarchy, commanding services or deference from all of the many other castes of their villages. Unilineal kinship attains a development which is extreme among Hindus in the Coorg *okka*, a unit which is identical with Emeneau's "sib" and equivalent to Murdock's "patri-clan," although it is termed "joint family" by the author. The elaboration of formal headmanship and higher political organization similarly finds parallels only in Malabar.

One special interest of the book is the fact that functional interpretation is extended to religious rituals of a complex society. Radcliffe-Brown in his foreword points out that Coorg society may be said to comprise three rather distinct structures—family, village and caste—each of which has its distinct set of rituals. Without forcing all of the data into any holistic interpretive formula, Professor Srinivas indicates how each ritual of the family and village cults may serve certain partial social functions. His main effort and achievement is to make us understand the religious idiom in its most immediately relevant terms. Conceptions of ceremonial purity, of which Coorgs distinguish no less than four degrees, are analyzed with much illumination.

At the same time, certain interpretive issues are not fully resolved. Social solidarity is analyzed according to the mechanical model almost to the exclusion of the organic model; this is perhaps inevitable in a study which takes a single caste rather than a community as its subject. An incipient distinction between solidarity which is achieved through common beliefs as contrasted with solidarity achieved through common cult acts is not carried out in the interpretation.

Questions of caste stratification receive only minor attention. The Coorg facts do not appear to agree well with the author's general explanation of ranking within the caste system, which holds that high status is determined by approximation to a Brahman way of life. The book contains little information concerning everyday economic rituals and deference gestures between the castes. It does not fully answer the intriguing questions raised as to the connection between ceremonial purity and structural distance. One is left therefore with the suspicion that power rather than way of life may have determined the standing of the Coorgs in relation to the ambiguous *varna* scale of values.

A useful approach is made to the staggering problem of the nature and growth of Hinduism. It is proposed to distinguish "Sanskritic" from folk elements in each peasant cult. Even from the perspective of exotic Coorg, the author holds that Sanskritic

rather than folk elements constitute the core of Hinduism as a whole, that the cults of higher castes are generally more Sanskritic, and that the ranking of castes promotes a filtering-down of Sanskritic traits from high to low. On the other hand, the local cults of village and region should show a relative preponderance of folk elements. Such propositions have heuristic value even if they cannot be demonstrated from evidence adduced in the present book.

On the whole, the book suffers most from its loneliness in a vast field. Professor Srinivas' recent intensive community study in Mysore may provide it soon with at least one worthy peer.

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Linguistics

Navaho Grammar. Gladys A. Reichard. (xiii, 393 pp., Publications, American Ethnological Society, No. 21, $7.00. J. J. Augustin, New York, 1952.)

Navaho is one of the few American Indian languages that has been sufficiently studied to produce what might be called schools of interpretation. Specifically, there is the Reichard school of one member, exemplified in the work under review, and the "Sapir school," as Reichard calls it, which seems to include everybody else. The reviewer is not himself a worker in Navaho, or any Athabaskan language, but as a linguist who has concerned himself much with both general theory and with a number of American Indian languages, he feels that he can legitimately examine this work and even take sides in the controversy.

Gladys Reichard got her early lessons in linguistics and in anthropology in general from Franz Boas. So did Edward Sapir. Harry Hoijer and Robert Young got their training from Sapir. The reviewer had the great privilege of participating in some of the last classes conducted by both Boas and Sapir. American Indian linguistics got its initial orientation from Boas. It developed into a comparatively rigid analytical science under Sapir and his contemporary Bloomfield and their students and associates and successors. Like any science, it did not stop just after starting. And the reverence due the great man who initiated it need not prevent later students from advancing far beyond his views. But Reichard seems to think that whatever Boas did not do should not be done, whatever Boas did not believe in should not be believed in, and whatever later scholars do that differs from Boas not only is not right but is somehow evil and irreverent.

The reviewer comes to the conclusions just voiced from examining Reichard's big book on Navaho. The chapter headings are: Introduction, Grammatical processes, Phonology, The word, The noun, The pronoun, Bound forms, The verb, The adjective, Prefixes, Syntax, Usage and vocabulary, Speech diversity. In the Introduction we are told about the "Sapir school of Athabaskan," and find out that Reichard has done a lot of deep thinking about linguistic analysis, but along lines that cause her to question everything that has happened in linguistics since about 1925. Grammatical processes is the heading for one short paragraph. The Phonology (pp. 13–45) makes clear that Reichard has resisted learning what a phoneme is, and how to handle the

phonetic facts in order to give a phonemic analysis. She confuses all levels in one: phonetics, phonemics, morphophonemics— all are treated in one jumbled mass of unorganized detail. Even that first pitfall to be avoided by any linguist—the confusion between language and writing—is found here, with Reichard falling squarely into it every time she discusses a symbol. In the matter of *x* and *h*, where the reviewer thinks Reichard has actually the better of it over Hoijer as far as details of data go, she fails completely to make sense of the data in terms that mean anything phonemically. The reviewer believes that *x* and *h* are probably two phonemes that are partially in free alternation, and this alternation seems to extend to position after a stop; Reichard's *tx* and *t* are most probably freely alternating clusters of a dental stop, to be written /d/ (or /t/), with /x/ and /h/; the behavior of /x/ and /h/ gives the clue to the phonemic analysis of the "aspirated stops," but since Reichard doesn't know how to do phonemics, she cannot point out this possibly real incompleteness in the Hoijer-Sapir analysis.

The rest of the book is a jumble of unalyzed lists and wrong analyses based on translation meanings. Without knowing a thing about Navaho itself, one could pick out scores of pertinent examples. Hoijer has noted a few in his review in *IJAL* 19, pp. 78–82 (1953). The present reviewer will refrain from laboring this point.

If Reichard's book had been published in the first volume of Boas's *Handbook of American Indian languages* (BAE-B40, 1911), it might have been one of the lesser contributions to that volume. But published in 1952 (or is it 1951?—the publisher of this series regularly omits that essential to evaluation of scientific work, the date) it is much worse than a minor contribution; it is a major error. One can only regret the vast amount of labor expended by the author, and the editing time and funds supplied by the publishing Society.

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PHYSICAL ANTHROPOLOGY

Die Negrito Asiens, I. Band: Geschichte, Geographie, Umwelt, Demographie und Anthropologie der Negrito. Paul Schebesta. (xvi, 517 pp., 64 plates, $12.00. St. Gabriel Verlag, Wien-Mödling, 1952.)

Dr. Schebesta would be among the first to state that the pygmy peoples of Africa and Asia are still an enigma; but any one familiar with his series of monographs on the Ituri-Bambuti would quickly add that no one else has done as much as Dr. Schebesta toward its eventual solution. The present volume is the first of a series that promises to do for the Asiatic pygmies what the earlier series has done for the African. This statement makes it appropriate to give Dr. Schebesta's final conclusions first: The Asiatic Negrito possess in common a basic pygmiform racial element—dark-skinned, curly-haired, brachycephalic, short-statured, with certain undifferentiated traits—upon which have been superimposed, in varying proportions, other racial elements (Melanesid, Veddid, Premongolid, Australid). Among the Aeta, the racial hybridizations tend to be localized, giving the entire group a stamp of localized differentiation;

whereas among the Semang the myxis is evenly distributed. The Negrito are not to be grouped with the African pygmies; but both of these ethnic bodies are diminutive, separate members of the Negrid "Formenkreis," although possibly there may be a genetic connection between the diminutive, lighter-skinned, brachycephalic Negrito racial element with peppercorn hair ("Type I") and the brachycephalic component among the Bambuti. It is this element which apparently is the bearer of most of the undifferentiated characteristics.

The painstaking résumés of the work of previous investigators would alone be worth the reading; but in addition, we have as the main body of the monograph the author's own findings from his trips among the Semang in 1924–25 and 1939, and among the Aeta in 1939. The reports on the Andamanese are derivative, and are therefore the least informative.

The development parallels that of the African series. With three separate ethnic groups to pursue and compare, the author explores each of these in turn as each topical heading is reached; eventually, he compares them, and also brings in the Bambuti as occasion arises. (In the case of the blood-groupings, he necessarily adduces also the average-statured peoples of the general Australasian region.) The topical development is as follows.

I. HISTORY, GEOGRAPHY, DEMOGRAPHY. Historical and geographical survey: the problem of the occurrence of Negrito groups and strains. The Negrito stocks proper. Discussion of nomenclatures and geographic locations. Occupances and cultural habitats, severally and comparatively. Demography. II. BIOLOGY, PHYSIOLOGY, PATHOLOGY. Blood-groupings. Growth. Somatoscopy and pathology (skin; eyes; hair; pulse and temperature; vital capacity; habitus; muscularity and movement; sensitivity; sleep; senses; voice; dentition; acturial data; etc.). Dermatoglyphics. III. MORPHOLOGY. Racial physiognomy. Somatometry, cephalometry; Osteometry. IV. POSITION OF THE NEGRITOS AMONG THE PYGMY PEOPLES. Racial composition, variation, myxis. Relations with neighboring peoples. Negrito and Bambuti: the pygmy problem. Three appendices register raw measurements, tables of indices, and measurements upon children.

This short review cannot do justice to all of the findings, and to the discussions of previous authors' data and theories. We offer only some sample conclusions. In Malaya, linguistic and mythological evidence corroborates the physical in the following set of hypotheses (p. 149): "1. Contact between Semang and the cave-dwellers described for the lower Neolithic is not verifiable. At all events, they were not the ancestors of our Negritos. 2. The Austronesians, bearers of the four-sided-ax culture [cf. Heine-Geldern—EWC], who *ca.* 1500 B.C. were widespread throughout Malaya, have left traces in the Semang languages. This may have gone hand in hand with a Malayo-Polynesian racial and cultural influence upon the Negrito, albeit not a significant one. 3. An older Mon-Khmer wave flowed over the Semang as well as the Senoi. Racial, cultural, and linguistic influences of this overlay have been very enduring and continue to operate today. 4. The influence of the later Mon-Khmer wave upon the Semang appears to have been insignificant, if present at all. 5. The Semang have been in contact with the Senoi since the immemorial, and have been exposed to these, culturally in particular, but also in part racially. 6. The Malayan imprint, which has long made itself felt

even in the depth of the forest, has left traces upon the Semang language and economy; but it has had no effect in the social and intellectual spheres."

In *demography*, the prognostication for the Aeta is rather good; for the Bambuti, even a little better; the Semang are headed for extinction; the Andamanese, rapidly so. The discussion of the causes, and the evidence, must be left for the book.

Serologically, we have the following alignment (pp. 196–198) 1. Australians, Maori, Mangyan are one serological unity; 2. So likewise the Melanesians and Micronesians, who are also close to the Hambal, the Atí, the Semang; 3. Malays, Indochinese, Annamites, Old Japanese, Dravidians, and Senoi form another grouping; 4. The Manidé stand by themselves; so do the Ongi (Andamans). Thus, among the Aeta the Hambal, Manidé, Baluga, and Atí all belong to different serological groupings. The split is even finer: the Zambales Hambal differ from the Bataan Hambal; the former being more hybridized. The Semang likewise subdivide into a northern and a southern grouping. Aeta, Semang, and Ongi are distinctive *inter se*. Nor do Bambuti and Negrito belong together.

Growth patterns also distinguish the Bambuti from the Negritos. The former are shorter and heavier; they are not malnourished, nor degenerate; their physique being a genetic matter. Semang children may be underweight for lack of sufficient food. Aeta and Semang resemble each other; their facial and cranial differences may reflect the differences in their intruded racial strains. The Negrito certainly are not of the same race as the Igorot and the Mangyan.

If the succeeding volume follows the plan of the Bambuti studies, we shall expect a monograph on the ethnography of the Negritos: economic, social, and religious culture. There is plenty of reason to expect that our patience will be well rewarded.

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Contributions to the Anthropology of the Faiyum, Sinai, Sudan, Kenya. HENRY FIELD. (352 pp., $4.00. University of California Press, Berkeley and Los Angeles, 1952.)

As Physical Anthropologist to the University of California African Expedition the author performed a feat in measuring, during five months, almost one thousand men in eight samples from the four areas covered: Tamiya and Fidimin *fellahin* from Faiyum; Beduin and Jebeliyeh from Sinai; Abka Nubians, Abu Hamed Rubatab, and Uum Disa Gumueye Arab-speakers from the northern Sudan; and eastern Masai from Kenya. It is truly amazing that in addition each of the four divisions of his book is a mosaic of both personally observed and quoted data on newly found Palaeolithic and later sites, tools, and pictographs, on each area's geology, geography, climate, ecology, history, ethnography, and even linguistics: everything from Woolley and Lawrence's description of Sinai aridity and the date yield in northern Sudan, to the distribution of camel brands and Sheikh Eid's opinions of his neighbors. Unity comes from narrative sequence rather than any central problem: aridity's selective force, Moslem-Western contrasts or the significance of Mediterranean-Negro interpenetrations remain merely implied problems. For Sinai Dr. Field notes "the modern dwellers on the land bridge between Asia and Africa appear to be close physically to the ancient Proto-Mediter-

ranean of Southwestern Asia and to the modern basic Mediterranean type of this area." This directs the focus of the whole book on a future critical study of ethnological and biological relationships between Southwest Asia and Northeast Africa; but for this purpose the author's carefully annotated field diary provides the lens of a bee rather than that of a bird.

Full notes, statistical tables, and selected bibliographies for each section are most valuable, including alternate transliterations for proper names. The Masai phrase book is likewise unique, though some of the idioms sound colonial. This exemplifies the difficulty in such a broad survey of treating subcultures of each area objectively and as wholes. Lack of time plus local prejudice also apparently prevented blood-typing or adequate study of physiology, body-build, or family relationships, though the trait frequencies will have value for future genetic approach. Minimum frontal breadth is over 5% higher than that of most workers. What are blue-ringed sclerae? *Arcus senilis*? Physical anthropologist, prehistorian, ecologist and social anthropologist will lament lack of photographs, of drawings, of population density and mobility diagrams, and of summaries of cultural activities of women versus men; all largely results of time and space limitations. Lack of sketch-maps is no greater handicap, though the reader must find something better than the standard atlases to follow.

In the light of the immense scope of the work, its implied aim, and the huge number of data and personal contacts carefully arrayed by the author, such lacks are trifling. Dr. Field's work is a major one, provocative as well as intensely informative.

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ARCHEOLOGY

Prehistoric Europe: The Economic Basis. J. G. D. CLARK. (ix, 349 pp., 16 plates, 180 text illustrations, $12.00. Philosophical Library, New York, 1952.)

For many years, archaeologists were satisfied with describing and classifying their material, setting up more and more periods and sub-periods, and establishing an increasingly detailed chronology, thus creating a framework within which their finds could be properly arranged. In large parts of the world, this work remains—and must remain—the chief task, because time-sequence is fundamental to all reconstruction of cultural development. On the other hand, it should be understood that classification is only a means to an end, since the ultimate goal of archaeological research must be an understanding of the forces at work behind culture change.

Needless to say, economy, in the widest sense of the word, is of paramount importance in this context. With respect to prehistoric Europe, Professor Grahame Clark has previously treated this subject in an imposing array of papers. Now, for the first time, he gives a complete survey of all the problems in question. Or, as he puts it, " . . . an attempt has been made to view this vast subject, as it were, stereoscopically, by bringing into focus two distinct lines of vision, those of the natural scientist and of the historian." As stressed by the author himself, the discussion must necessarily be somewhat unequal. Owing to lack of adequate source material, the USSR is underrepresented, while conditions in Northern Europe, where prehistoric research is, perhaps, in some respects further advanced than in certain other parts of the Old World,

are emphasized to a degree which is hardly justified by their importance to the general history of European culture. However gratifying this circumstance may be to a Scandinavian reviewer, it must, of course, result in a somewhat distorted picture.

As it is, Professor Clark's work nevertheless makes a highly inspiring contribution to European archaeology as a whole. He takes for his starting point the fact, probably too much overlooked by prehistorians of the older generation, that culture must always tend towards establishing some sort of equilibrium with environment. The book is therefore properly introduced by a description of the ecological zones and ecological stages during the prehistoric period, followed by chapters on hunting, food-gathering and farming. In the opinion of a non-archaeologist, this discussion is the most interesting part of the whole work, because the interplay between biomes, habitat, and culture appears with striking clarity. To mention but a single example: during the sub-atlantic period of climatic deterioration, farming, which in the Norwegian neolithic had extended beyond the Arctic Circle, retreated to the southernmost parts of the country in the early Iron Age. In the subsequent chapters on shelter and technology, the correlation between cultural phenomena and geographical conditions is somewhat obscured, although, for instance, the adaptation of house forms and types of settlement to the natural surroundings would seem to offer a tempting field of investigation. On the other hand, the ecological viewpoint reappears very clearly in the concluding chapters on trade, travel, and transportation.

There is every reason to congratulate the author on the completion of a work which could be accomplished only by a scholar possessing his extensive field experience and vast knowledge of the pertinent literature, written in many different languages and scattered in numerous periodicals, often difficult of access. Is it too much to hope that before long, our knowledge of pre-Columbian North America may be sufficiently advanced for a treatment along similar lines, by an archaeologist of Professor Grahame Clark's caliber?

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Danske Oldsager (Danish Antiquities). II—*Yngre Stenalder* (Late Stone Age). P. V. GLOB (139 pp., 70 plates, 1 map, Danish kr. 35.00. Gyldendalske Boghandel, Copenhagen, 1952.)

Danske Oldsager (Danish Antiquities). III—*Aeldre Bronzealder* (Early Bronze Age). H. C. BROHOLM (65 pp., 50 plates, Danish kr. 20.00. Gyldendalske Boghandel, Copenhagen, 1952.)

These outstandingly fine volumes are the second and third in a series of eight edited by Therkel Mathiassen and compiled by the staff of the National Museum of Denmark to supersede Sophus Müller's "Ordning af Danmarks Oldsager" (The Arrangement of Denmark's Antiquities), published in 1888–1895). The purpose of this series is to provide descriptions and illustrations of the leading archaeological types of the country from Mesolithic times to the Viking Period.

Volume II, prepared by P. V. Glob, is a truly magnificent pictorial work, in which the entire range of antiquities assigned to the Danish Late Stone Age is depicted. In all 637 objects are shown on 70 plates, each of which is fully described in the text.

The latter is divided into three main sections covering the Early (*ca.* 2500–2200 B.C.), Middle (*ca.* 2200–1700 B.C.) and Late (*ca.* 1700–1500 B.C.) Neolithic Periods, with an introductory orientation in each case giving the distinctive features and distribution of the various cultures. In addition, the volume contains a key to Sophus Müller's "Arrangement," as well as a map showing the districts and counties of Denmark, which are given for each figured specimen in the Danish text, together with the bibliographic references.

Essentially the same format is followed with respect to volume III by H. C. Broholm in his treatment of Early Bronze Age culture in Denmark. This begins in the middle of the second millennium B.C. and includes only the Second and Third Periods of Oscar Montelius' six subdivisions of the Scandinavian Bronze Age, since it is now clear that Montelius I falls within the final phase of the Late Stone Age. But in Montelius II a native culture arose in Denmark with its own special types clearly distinct from the imported bronzes. Broholm recognizes three major periods of Danish Early Bronze Age culture, which commenced *ca.* 1500 B.C. and gave way to the Late Bronze Age by *ca.* 950 B.C. He illustrates a total of 357 objects on 50 plates, carefully selected to show the range of types of this cultural development. One is struck by the uniformity of the prevailing artifacts throughout Denmark at this time. Furthermore, when it is realized that every ounce of metal had to be imported, the number of locally-produced objects is astonishing. Settlement sites of this period are unknown, and our knowledge of the culture is limited to grave finds and hoards, as well as stray objects found in fields and peat-bogs.

The advanced student of European archaeology interested in distribution studies will find that all the data necessary for the compilation of distribution maps are provided in both volumes. In works of this kind, however, the omission of such maps, as well as of pictures of some of the main types of sites referable to the various cultural stages, is difficult to understand. Essentially these books are beautifully illustrated and fully annotated corpuses of Danish Neolithic and Early Bronze Age antiquities. They are indispensable for teaching purposes, with an enormous wealth of very fine illustrative material to choose from in making slides. Certainly these books will be of great value as works of reference for many years, and by reducing the text to statements of fact and omitting detailed descriptions the authors have performed an immense service to archaeology. Finally, the Carlsberg and Rask-Ørsted Foundations should be congratulated for so generously subsidizing the cost of publishing these profusely illustrated and bi-lingual works at such a reasonable price.

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OTHER

Agricultural Origins and Dispersals. CARL O. SAUER. (110 pp., 4 maps, $4.00. The American Geographical Society, New York, 1952.)

Dr. Sauer is a geographer well known for his stimulating ideas about the relations of man to his environment. In the lectures which make up this book, he presents his hypotheses on the origin and spread of cultivated plants and domesticated animals.

Dr. Sauer believes that man originated before the Pleistocene, and that by the early Pleistocene he was spread widely over the Old World and knew how to use fire. He considers the optimum period for man's migration to the New World to have been during the Third (Illinoian) glaciation. Long before man had learned to disturb vegetation by his plantings and concentrations of domesticated animals, he was changing plant formations through his use of fire for hunting and gathering. Repeated burning of the plant cover produced and maintained the prairies and savannas.

It is suggested that the earliest agriculture was practiced by sedentary folk, probably fishermen in wooded areas of Southeastern Asia. This first agriculture was based on vegetatively propagated plants, many of which originated in this region. The starchy earth crops would have formed a desirable addition to a diet already rich in proteins. In the New World, too, Sauer believes that agricultural beginnings were founded on root and tuber crops. He prefers to see Colombia and Venezuela as the center for these early efforts. After explaining his hypothesis that "both tropical Asian and Andean planting cultures are environmentally divergent developments of one basic . . . way of growing and improving plants," Sauer gives some general statements in support of a diffusion of Old World culture to the New.

The development of seed agriculture is considered to be later than the use of the earth crops. In the Old World three main early areas of seed agriculture are distinguished: North China, western India to the eastern Mediterranean, and Ethiopia—each with assemblages of cultivated annuals grown for seeds rich in starch, proteins, or oils. In these areas, again, the first plantings are considered to have been made in mixed or wooded areas, not in grasslands. For the New World Sauer favors the Mexican-Central American border as the hearth of the principal seed plants. In conclusion he points out that man's use of domesticated plants and animals destroys the soil cover and is the major cause of our growing desert regions.

It is possible that agriculture in the Old World began with the cultivation of the earth crops as Sauer suggests, and the three seed hearths he delimits are proven centers of distinct assemblages of seed plants. It is unfortunate that so much space is spent on a discussion of transpacific movement at this time, since there is no good evidence either for or against such movements of cultivated plants in pre-Columbian times. I do not believe that the transpacific carriage of cotton, sweet potato and coconut is proven or disproven. Even the relationships of the pre-Columbian varieties of the New World gourds to those of the Old World are unknown. There are many other plants which invite research. Of five which Sauer suggests, the sweet flag and edible nutgrass were certainly present in both hemispheres, yet these cannot be considered significant unless they are known to have been cultivated. The cattail is found and eaten in both hemispheres but as far as I know is not cultivated.

Venezuela is suggested as the home of manioc, but the greatest diversity of the cultivated forms and of the wild relatives is found far to the east in arid northeastern Brazil. The author dismisses the Amazon basin as a center for New World agriculture, yet in this basin, on its western edge to be sure, are found most of the great agricultural developments of South America. Maize use in South America is termed inefficient, yet corn is used to make numerous food products, even sugar by a process similar to that employed by our great corn refining industry. A large part of the corn is made

into chicha. This product supplies, through its yeast, nutrients otherwise scarce in the diet.

It is odd that an area in which originated two species of tobacco, coca, datura, quinoa, kañahua, *Canavalia*, *Cyphomandra*, pepino (*Solanum muricatum*), the flowering nasturtium, at least one kind of pepper, a lupine, and perhaps one species of squash, should not be considered a more likely center for the origin of seed agriculture, especially when the llama and alpaca were domesticated there. (Sauer states that herd animals belong with seed agriculture, the reindeer being the sole exception.)

These are minor differences of opinion at a time when hypotheses like Sauer's can suggest new avenues of research in a field where little has been done. The book is written in a fashion which should interest most intelligent readers, as well as students of man or his domesticated plants and animals. It is unfortunate that the price is not lower, and that an index is lacking.

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The Nature of Historical Explanation. PATRICK GARDINER. (142 pp., $2.00. Oxford University Press, London, 1952.)

A philosopher here analyzes the explanations provided by historians and argues down some views that have been advanced as to the nature of historical knowledge and method. It is the history of historians that is in view; little is said of social science; anthropology is, I think, not mentioned. Yet there is much here to help an anthropologist to understand his own work.

Explanation in history, says this philosopher, is not to be falsely assimilated to explanation in natural science: validation of a generalization. Historical explanation is an assessment, not a definitive conclusion; it is a sketch, showing the directions in which factors that explain are to be found. Generalizations and concepts there are, but these are vague and open, "able to cover a vast number of events fully within an indefinitely circumscribed range." The generalizations are there, in the background, to guide understanding of particular events. "Historical explanation is a game in which there are no clearly formulated rules; the more we tighten the rules, the more difficult it is to play the game." Some historical explanations are causal, connecting an event with others before it. Others relate events to men's motives or other mental states, and so "set John's action within the pattern of his normal behavior." This is another kind of explanation.

Thinking of anthropology from this viewpoint, one sees various anthropological works as approximating this description, or as departing from it, science-ward. Gardiner recognizes "a 'slide' from explanation as it occurs in history." Boas' explanation of the designs on Alaskan needle-cases uses several vague, commonsense generalizations as to the making and modifying of design to explain the needle-cases. Kluckhohn's explanations of Navaho witchcraft has moved nearer the scientific model, but yet has much of the looseness of the historian's "game." Is there not an "assessment," a judgment formed rather than a definitive conclusion reached, in a functional analysis, or in an explanation of the interrelations of institution in forming a modal personality,

rather than such a proof or disproof as a physicist would reach? When an anthropologist explains an action by setting it within the patterns of that culture, he explains, non-causally, as does often the historian. This book of Gardiner's may be used to help us to see ourselves as we really are. "There has been a . . . tendency to be impressed (and obsessed) exclusively by scientific explanation," writes Gardiner.

There are other passages significant for us. On pp. 120–133 appears a simple analysis of the ethnologist's justification for presuming to say that he knows what a native feels and thinks. True, the topic is not so announced; it is discussed in terms of the historian's claim to know the mind of Napoleon. But the problems are the same, with the added difficulty that Napoleon is dead. The book ends with a brief destruction of the apparent antinomy between materialist and idealist interpretations of history.

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Science, Language, and Human Rights. (Papers for the symposia held at the annual meeting, at the College of the City of New York, December 29–31, 1952) (211 pp., $2.50. American Philosophical Association, Eastern Division, Vol. 1, University of Pennsylvania Press, Philadelphia, Pennsylvania, 1952.)

Ten papers distributed over five symposia make up the contents of this slim volume. The subjects of the symposia are phenomenalism, problems of concept and theory formation in the social sciences, rules of language, the concept of expression in art, and the concept of universal human rights. Of especial interest to this reviewer are: Roderick Firth's verbal juggling in an attempt to demonstrate the necessity of the "sense-datum language" for epistemological purposes; Max Black's counter-argument that "it is well-known that logical transformations can be performed upon sentence-forms containing variables, or upon the same sentences with nonsense-words substituted for the variables" and his rejection both of phenomenalism and of the sense-datum language needed for formulating it; Ernest Nagel's theses that "knowledge involves the discovery through processes of controlled inference that something is a sign of something else," that "it is statable in propositional form," and that "it is capable of being verified through sensory observation by anyone who is prepared to make the effort to do so"; that comprehensive sociocultural formulations are valuable only to the extent that they effect "a systematic organization of manifest data obtained from overt human responses in a variety of conditions, and only in so far as [they make] possible the formulation of regularities in such responses," and to the extent that they do not depend on "the vagaries of special insights and private intuitions." Such prescriptive generalizations for the social scientist are of particular interest to the linguist who has begun to put this kind of reliability into his work on one aspect of human culture. These, together with Carl Hempel's interdiction of the *ceteris paribus* clause, in his denial that ideal types constitute anything more than "an unimportant terminological aspect, rather than a distinctive methodological characteristic, of the social sciences," constitute a refreshing profession of faith in the future of a science of man. R. M. Martin's plea for "one broad, comprehensive semantical meta-language in which object-languages themselves might be taken as values for variables" and his suggestion that "we should have to be much more precise than heretofore as to

what language-systems are", are a desideratum and a conviction expressed by a number of contemporary descriptive linguists who deal with natural languages and who look to the philosopher-logician for help in solving a similar problem. It would seem to the horse-sense of the cultural theorist that the musical "sadness" discussed at some philosophical length by Vincent Tomas and Douglas Morgan could be said to reside only in the psycho-physical disposition of the human being, although certain objectively determinable characteristics of the music might be found to trigger-off the human disposition. The moral dispositions discussed by A. I. Melden and W. K. Frankena would likewise seem to reside in particular human beings and to there await discovery by the investigator. Whether or not any one of them, such as the concept of "universal" human rights, merits the label, would seem to constitute an empirical question, to be answered, on the one hand, by the investigation of the attitudes of an adequate sample of human beings, or, on the other, by the discovery of the disposition in one human being to impose the concept on his fellow humans. If he were sufficiently persuasive, might he not succeed?

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The Taming of the Nations: A Study of the Cultural Bases of International Policy. F. S. C. NORTHROP. (xii, 362 pp., $5.00. The Macmillan Co., New York, 1952.)

This is a significant book by the author of *The Meeting of East and West*, carrying forward the main theme of that volume—the exploration of the ways in which the cultural gulf separating the East and the West can be bridged. The treatment in the present volume, however, is more pragmatic, with the objective shifting from a scrutiny of the theoretical possibility of "world understanding" to the practical feasibility of formulating an international policy which might secure global peace.

The two regions of the world which must attain understanding are Asia (defined as the Indian sub-continent, South-East Asia, China and Japan) and the Euro-American world. At the same time, however, the horizon is widened, and it is recognized that "there are seven major cultural political units in the contemporary world" (p. 286), namely Asia, Islam, Africa, Europe, the British Commonwealth, Pan-America, and the Soviet communistic world. Signs of closer cooperation between some of these major units or parts of them (such as the U. S., and Great Britain and Canada) are noted and hopefully taken to indicate a trend "toward a single world ideology" (p. 308).

Northrop feels that nationalism is on the decline in most of these major world units, to be replaced by a "rise and resurgence of culturalism" (pp. 178, 277). Specifically, he notes this movement in Asia, Islam, Africa and Europe (pp. 277–84). Thus, a relatively small number of different cultural identities is recognized whose ideals must be "grasped from within" in order to be able to formulate an effective foreign policy and ultimately "an effective world law" (pp. 277, 306–7).

According to Northrop, this effective world law can be achieved if it is recognized (following Eugene Ehrlich) that each of the major cultural-political units has a "living law" of its own (that is a complex of moral and legal ideas), which will not be affected or changed by signing any international charter. A world law, therefore, must "guarantee to each ideology and nation of the world protection of its particular norms in its

own living law geographical area" (p. 271). The international law will regulate international relationships, but will have no right to interfere with the working of the internal living law of any nation (p. 272). Here one cannot help wondering, how far this admission in principle of the internal validity of the "living law" of each nation can be reconciled, especially in the case of dictatorships, with the international concern with human rights and civil liberties of certain groups or individuals living within their geographical areas.

The major part of the book is devoted to an analysis of the cultural identity of Asia (pp. 28–148), with a brief chapter dealing with the resurgence of Islam in Pakistan (pp. 149–72). He believes that in its relations with Asians, the United States must make it clear "that we value and prize" their "particular cultural values and traditions" if Asia is to be freed from the Communist lure.

A fascinating sequel analyzes the cultural bases of India's repeatedly manifested aversion, not only to the use of force (as in the police action of the U. N. in Korea) for the settling of disputes, but even to the use of determinate rules for the same end (p. 80). The Asian ideal is intuitive mediation, compromise, the middle way which satisfies both disputants. In its foreign policy, India is deeply influenced by these concepts, although when faced with internal chaos after partition, it has had to resort to the use of armed force. Only if Asia can be made to realize that in world-relations as well "some uses of force are not evil" (p. 335), will the joint and augmented police force of the East and West together be strong enough to deter any aggressor, even one as powerful and cunning as the Soviet Union. How Asia's change of mind can be specifically effected is not stated, although it is the author's hope that it can be brought about (pp. 334–5).

Implicit in Northrop's thesis (as adumbrated in *The Meeting of East and West*, and developed in *The Taming of the Nations*) is acceptance and reliance on the anthropological theory of cultural relativism. New and significant is his attempt to carry this theory to its logical conclusion and to predicate upon it a practical scheme for the attainment of a more nearly frictionless relationship between cultural-political units of greatly differing character.

RAPHAEL PATAI

DROPSIE COLLEGE
PHILADELPHIA, PENNSYLVANIA

Flour for Man's Bread; A History of Milling. JOHN STORCK and WALTER DORWIN TEAGUE. (xiii, 382 pp., 131 figs., $7.50. University of Minnesota Press, Minneapolis, 1952.)

This history of wheat milling was prepared from materials assembled for a Museum of Milling History projected by a Minneapolis milling company about 1945 but abandoned in 1947. Dr. John Storck, historian and author of *Man and Civilization*, did the research while Mr. Walter Dorwin Teague, an industrial designer of wide experience, drew the plans. The authors show that the history of civilization is closely linked with the development of methods of cereal preparation. As grinding methods advanced from simple mortars to saddlestones, push and lever mills, the hourglass mill, querns and millstones and thence to the complicated methods of our modern roller mill, man progressed from a gathering and hunting existence to stable community life in

which individuals became less independent. On a primitive saddlestone, for example, a Sudanese slave working from morning to night could make meal for eight persons for one day. An 18th century quern was at least twelve times as productive. And a modern miller can, in 20 minutes, produce enough flour to feed the average American for a year. While much has been written concerning the value of agricultural advances which now enable 8 people to live off the land for each one working it, milling improvements have been far more effective in saving time for pursuits other than the acquisition of food.

More than half of the book is devoted to the development of the modern American milling industry. Most of this section illustrates the gradual improvement of the mills and gives diagrams and details of the equipment, and I know of no other source which so clearly documents the relation of wheat mills to changes in transportation and agriculture and to westward expansion of the United States. Some of the information in earlier chapters is to be found in H. E. Jacobs' very readable *Six Thousand Years of Bread, Its Holy and Unholy History*, but Jacobs spends much of his space on bread and gives relatively few details of mills and of the great American achievements.

Anyone interested in the origin of the cultivated wheats will find the explanation in Edgar Anderson's *Plants, Man and Life*, easier to understand and far more suggestive of the relation between the development and dispersal of the wheats and man's activities. The title, *Flour for Man's Bread*, is ambiguous because rye, barley, oats and corn are barely mentioned. The few illustrations of American Indian corn milling equipment do not even include the South American rocker stone. The chapters on research and the future of milling are thought-provoking but the authors are a bit overenthusiastic about the ability of the iron and three vitamins added to flour labeled "enriched" to take the place of more than a dozen vitamins and other substances required by the human body which are to a large degree removed with the germ and outer layers of the grain. These are minor points in a text which covers so large a subject so effectively. Footnotes to some of the details would have been handy but, the authors point out, so many scattered references were used that footnotes and complete citations would have made the volume too bulky. A list of selected sources, a vocabulary of milling terms, and a brief index are provided.

HUGH C. CUTLER

MISSOURI BOTANICAL GARDEN AND
WASHINGTON UNIVERSITY
ST. LOUIS MISSOURI

BOOK NOTES

Science in Alaska: Selected Papers of the Alaskan Science Conference of the National Academy of Sciences, National Research Council. Edited by Henry B. Collins. (iv, 305 pp., $2.25, Arctic Institute of North America, Washington, D. C., 1952. Copies on sale at Office of the Arctic Institute 1530 P St., N.W., Washington, D. C.)

This publication is the result of the first Alaskan Science Conference held in Washington, D.C., November 1950. The proceedings of the Conference were published by the Academy Council in April 1951 and included abstracts of other papers presented at the Conference. This volume contains twenty-three papers, six of which are under the heading of Anthropology. The others are Agriculture and Forestry, Botany, Geology and Geography, Geophysics, Meteorology, Public

Health and Medicine and Zoology. Anthropologists may find the papers on Botany, Geology and Zoology well worth reading if they are concerned with any type of anthropological reaserch in Alaska. Two of the anthropological papers, by Viola E. Garfield and Margaret Lantis, summarize the present status of research in cultural anthropology in Southeastern Alaska and the region of the Eskimos. Two of them, by Frederica de Laguna and Diamond Jenness, discuss the preservation of archaeological remains in Alaska and Canada, pointing up the difficulty of any practical control; and the other two papers, by Laughlin and Giddings, are more theoretic and concerned with specific anthropological problems in the North. These last two are thoughtful statements resulting from extensive field work in the Aleutians and the Bering Strait area. Both are original contributions in Alaskan anthropology and will be of primary interest to readers of The AMERICAN ANTHROPOLOGIST. Henry Collins has done a commendable job of editing. Nevertheless, your reader wonders whether the theory of treating a region like Alaska as a separate, discrete field of research is actually sound. The interrelation of all the researches contained in this volume seems somewhat tenuous. (FROELICH RAINEY)

The Indians of California in 1852. JOHN WALTON CAUGHEY (xxxiv, 154 pp., $3.50. Huntington Library, San Marino, Calif., 1952).

Caughey's book consists primarily (pp. 1–69) of the carefully annotated text of the report of 1852 actually written by Benjamin Hayes, but attributed to B. D. Wilson, who was sub-agent for Indian affairs in Southern California. The report was first printed in 10 installments of the Los Angeles *Star* in 1868. An introductory section (pp. i-xxxiv) by Caughey presents the background to the Wilson Report by establishing authorship, biographical data and the contemporary scene. The final section (pp. 71–154) is a selection of documents, in the form of repriting relevant Indian matters from 1850 period newspapers, and official correspondence over Indian affairs taken from the Bancroft and Huntington Library archives. The Wilson Report is a notably explicit analysis of the sociological problem presented by the California Indians who were still either living the aboriginal life or had been released from Franciscan domination with the secularization of the missions less than twenty years before. Wilson proposed a reservation system, recommended reserve areas, and supplied detailed data on their management. One successful, though prematurely terminated reservation (Tejon) was started by E. F. Beale, then superintendent of Indian Affairs, but the success of the California Senators in coercing the U. S. Senate not to ratify the famous 18 treaties of 1851, and a rapidly expanding and largely disinterested Caucasian population in California soon established the attitude of indifference toward the Indian problem that prevails even to this day. Caughey has done an excellent job with an important subject. The few errors in the bibliography, such as mistitling Powers' 1877 book, *Tribes of California*, and attributing the 1933 edition of Boscana's *Chingchinich* to M. R. instead of J. P. Harrington are minor ones. (ROBERT F. HEIZER)

Tupari; Unter Indios im Urwald Brasiliens. FRANZ CASPAR. (217 pp., 20 pls., sketch map. Friedr. Vieweg & Sohn, Braunschweig, 1952.)

Eager to meet an Indian tribe not yet thoroughly spoiled by Caucasian influences, the young Swiss ethnographer Caspar ascended the Rio Guaparé and its affluent, the Rio Branco, finally marching through the primeval forest until he reached the settlement of the people known as Tupari. These he ultimately discovered (p. 215) to be an amalgam of genuine Tupari and several other minute tribelets, with Wakaraü numerically predominant. Reserving monographic description for a future issue of the series of the Hamburg Museum für Völkerkunde, Herr Caspar here offers a lively account, spiced with humor, of his experiences with this group during a six months' stay in 1948. He succeeds in giving a graphic picture of the daily routine, his active participation in horticultural labors, the chicha sprees, with attendant brawls, his narrow escape from the conjugal snares set by the chief's cousin. Withal the reader finds an appreciable number of ethnographic

details. Of the forty surviving families, thirty were housed in a single maloca, hammocks being slung from the posts along the wall (p. 83). The importance attached to peanuts among cultivators of yuca, maize, and bananas is noteworthy; in size they exceeded those the author had hitherto encountered either in Europe or South America (pp. 105–107). He was permitted to attend some rituals and even to photograph spiritistic séances (p. 175 et seq.). The dietary and other restrictions for a bride are noted; five seems to be the mystic number (p. 192 f.). The Mongolian spot is so general that the natives refused to believe Caspar when he asserted its absence among white infants (181).

Herr Caspar is evidently a very good observer and enters sympathetically into aboriginal life. Americanists will look forward to the more technical report he promises us. (ROBERT H. LOWIE)

Bloodstoppers and Bearwalkers. RICHARD M. DORSON. (305 pp., $5.00., Harvard University Press, Cambridge, 1952.)

This is a fine collection of oral folk traditions gathered by the author, who is an historian and folklorist, during a five-months field trip to the Upper Peninsula of Michigan. The past century of this country has seen the exploitation of its timber, copper, iron, farmlands and fishing grounds, and the development of a rich and varied local lore. A dozen European nationalities jostle each other in every town, there are Ojibwa and Potawatomi, and there are Yankees. The book follows this cultural division, and material is presented in the narrator's own and often lively idiom. Part I, The Indian Tradition, speaks of the fear of bearwalkers, the magic of love powders, and the excrementitious adventures of Winabijou. In Part II, the European Tradition, the French-Canadians tell of the terrors of the *loup-garuo*, Cousin Jacks (Cornishmen who came to work the Peninsula mines) pride themselves on the durability of their beloved pasty, Finns boast of their strength, and almost everybody tells of the power of bloodstoppers. Part III, The Native Tradition, is full of tales of the brawling boom days: the lynching of the McDonald boys, the prodigious drinking and fighting of the lumberjacks in town, the cave-ins in the mines, and the belief of lakesmen that Superior never gives up its dead. The book concludes with a useful annotated bibliographic appendix. Mr. Dorson obviously liked collecting these tales, and thoroughly enjoyed working with the wide variety of people he met. The value of his book is enhanced because he chose to let his people speak in their own words, and thus the traditional tales sparkle with a freshness of expression. (WILLIAM CAUDILL)

La Familia Rural Haitiana. REMY BASTIEN. (184 pp., 2 illustrations, 2 maps, Libra, Mexico, 1951.)

This study, the author's doctoral thesis at the Universidad Nacional Autonoma de Mexico, is the most substantial report yet to appear on the Viking Fund sponsored ethnographic research undertaken by Dr. Alfred Metraux and a group of Haitian ethnographers in the Valley of Marbial in southern Haiti. The ethnographic survey was planned as a contribution to the Haitian-UNESCO Pilot Project set up for that area in 1947, and this study reports on six months of field work in 1948. Bastien has transcended the limits of his assignment and produced a solid ethnographic report on the Haitian family in the Marbial area against a broad socio-economic and historical background, but a report sensitive also to the action problems to which the Pilot Project is oriented. The study focuses on a structural and functional interpretation of the family, and especially the *laku* (French: *la cour*), the residential extended family grouping, and the analysis is a model of perceptive and sophisticated, if somewhat eclectic, anthropological research. The rise and fall of the *laku* as a social institution is interpreted and explained in historical and theoretical terms which give the work far more than merely regional interest. (MUNRO S. EDMONSON)

Beiträge zur Völkerkunde Nordamerikas. ROBERT H. LOWIE. (68 pp. Mitteilungen aus dem Museum für Völkerkunde in Hamburg, XXIII, Hamburg, 1951.)

The four articles in this publication (1. "Gegenwartsströmungen in der amerikanischen Völkerkunde"; 2. "Die Religion der Crow-Indianer"; 3. "Eigentumsrecht und Zwangsmacht bei Militärgesellschaften der Prärie-Indianer"; 4. "Einige soziologische Probleme der Völkerkunde") were originally given by Professor Lowie as lectures at Hamburg during 1950/51. As far as this reviewer has been able to determine these articles do not present any original material; in fact they appear to be summaries and paraphrases of materials already published by Lowie in English. For example, the first article contains the author's well-known evaluation of Boas and his followers, his equally well-known critique of configurational studies, his high valuation of correlation studies, etc. His omissions are also characteristic. For example, compared with some hundred lines devoted to Boas there appear only brief mentions of Sapir, Dixon, and Kluckhohn. The second and third articles represent summaries of well-known works by Lowie on these subjects in English. The fourth contains brief discussion on *Avoidance*, *Matriliny*, and the *Origin of the State*. The main significance of these papers is that they bring to a wider audience some of the results of Professor Lowie's long and productive work in Ethnology. (DOUGLAS OLIVER)

A Arte dos Índios Kadiuéu. DARCY RIBEIRO. (43 pp., 53 figures, Serviço de Documentaçáo, Ministério da Educação e Saúde, Rio de Janiero, 1952.)

The graphic and plastic art of the some 300 remaining Kadiuéu, Gauicuruan-speakers of southern Mato Grosso, Brazil, is excellently described in this work. Although both men and women are artists, the latter excel and their artistic tradition goes back much farther in Kadiuéu history than that of the men. Women work in painting (body, leather and pottery), tatooing, ceramics, and weaving; men carve wood and horn, cut and hammer metal, and, along with women, model figurines in wax, clay and cork. With this one exception the motifs produced by women are geometric and abstract; those produced by men tend toward realism. At present, according to Ribeiro it is in their art, more than any other cultural facet, that the Kadiuéu take great pride; there is considerable rivalry, particularly among the women, to be recognized as an excellent potter or body painter or tatooer. From the numerous plates, as well as the discussion of techniques, history, and acculturative factors, the reader gets a vivid and complete picture of contemporary Kadiuéu art. (VIRGINIA WATSON)

Earliest Evidence of Banana Culture. PHILIP K. REYNOLDS. (v, 28 pp., 8 plates, $1.00. Supplement no. 12 of The Journal of the American Oriental Society, Baltimore, 1951.)

The earliest records of banana culture the author found, in the Epics and the Pali Buddhist canon, are only about 2,500 years old but he considers it not improbable that the banana was a human food in southern Asia long before plants were cultivated. The banana probably reached Africa about the same time as the first Arab traders and these Arabs helped to spread it across the continent. The word *banana* apparently comes from the Guinea Coast while the scientific name *Musa* is derived from the Sanscrit *Moca*. Bananas started to spread eastward from Asia about the time of Christ and were widely distributed in the Pacific by the time the first explorers arrived. The first bananas in the New World were brought from the Canary Islands to Santo Domingo in 1516 by the Dominican Friar Berlanga. (HUGH C. CUTLER)

Ancient South-east Asian Warfare. H. G. QUARITCH WALES. (i, 206 pp., 7 figures, £ 1/15. Bernard Quaritch, Ltd., London, 1952.)

The hope of the author in writing his scholarly account of ancient warfare in southeast Asia is to provide insights into "national character and civilization" such as are not provided by the arts

of peace. He begins with what he thinks must have been the methods of primitive warfare prior to Chinese and Indian times, resorting to materials from the Torajas of Celebes in the belief that their culture has not changed much in the past 2,000 years. He then proceeds by indirection to show what must have been the influence of the Chinese on the area, this being very old (as least 3rd century B.C. in Borneo and Flores) but ephemeral. Indian cultural influences came next and were strong, leaving their military imprint in the form of expansionist policies, heroic ideals, and method of waging war. The basis of Hindu warfare was religious, war being regarded as a sacrifice. and there was much recourse to divination, particularly astrology. The earliest reference to war in Srivijaya is an inscription data A.D. 683, after which there are various written sources which chronicle Sumatran methods of battle and conquest. These were Indian in nature, as was warfare among the Sailendras in Java of the 8th century, A.D. The Javanese of the Majapahit empire continued to practice Epic style warfare. As for the Khmers and Chams of the 5th to 8th centuries A. D., we have from the Chinese information regarding the Indianized character of their warfare, and for later periods we have various inscriptions and bas reliefs, such as at Angkor Wat. Wales' description of ancient Siamese and Burmese methods of warfare is inferential, being based largely on isolating out from the usages of more recent centuries those which probably preserve the earlier Indianized methods. This book is highly descriptive and too brief. Interpretation would have made it of far greater value, although it is useful to ethnology as it stands. (WILLIAM A. LESSA)

History of the Luapula. IAN CUNNISON. (viii, 42 pp., 4 pls., 2 maps, $1.00, Rhodes-Livingstone Papers, No. 21, Oxford University Press, New York, 1951.)

This very attractively-written little paper describes the place of folk history in the lives of the people of the Luapula Valley on the Northern Rhodesia-Beligan Congo border. The author, an anthropologist attached to the Rhodes-Livingstone Institute, has clearly achieved a good deal of insight into Luapula modes of thought, and apparently was able to carry on his field work through the local language.

What is perhaps the main conclusion of the paper, that folk history is a charter for the present-day social order, is of course no new idea. But the author goes a good way beyond this in pointing out how this effect is achieved in particular Luapula idiom. The complete identification of mythical figures with their present-day heirs, the feeling that history may be "spoiled" by being related by a person who is not its proper "owner", the subordination of absolute time to the relative importance of events, the primacy of human motives with supernatural forces entering only to judge: all these are features which students of Bantu cultures will recognize and appreciate. The account is completed by an analysis of the differences between the histories of particular social groups, histories of the valley as a geographical entity, and dated history, which has been introduced with the art of writing.

The author is to be commended for reaching a new subtlety of insight in an area of the world in which a rather austere structuralism has hitherto dominated anthropological research. (LLOYD FALLERS)

Marriage in a Changing Society. J. A. BARNES. (ix, 136 pp., 14 tables, 4 illus., $2.25, The Rhodes-Livingstone Papers, No. 20, Oxford University Press, New York, 1952.)

This study of marriage among the Ngoni was undertaken as a contribution to the African Marriage Survey organized by the International African Institute and the International Missionary Council. The purpose of the book is to compare the marriage institution in the period just prior to intensive contact with the British (1890's) and in the present, to indicate the nature and processes of the changes, and to delineate the relationship between marriage and other aspects of society. After brief discussions of pre-contact marriage and the agencies of change and a more detailed analysis of the structural and behavioral relationships between husbands and wives and between kinsfolk, affines, and neighbors, Barnes summarizes the changes consonant with the

reduction of the authority of chiefs, changes in the structure of the villages and of kinship, the teaching of Christian missions, and other external influences. This analysis, excellent in itself, will find a further value in the context of the African Marriage Survey and in other cross-cultural analyses of institutions under conditions of intensive contact. (ROBERT A. LYSTAD)

Recueil de Textes Falachas. A. Z. AEŠCOLY. (284 pp. $7.00. Université de Paris. Travaux et Mémoires de l'Institut d'Ethnologie, LV. Institut d'Ethnologie, Paris, 1951.)

This volume covers much the same ground as Wolf Leslau's *Falasha Anthology*, discussed in the *American Anthropologist* (54: 569, 1952), likewise availing itself of manuscripts not utilized by or not available to Halevy in his *Tĕ'ĕzāza Sanbat* (Paris, 1902) and other publications on the literature of the Falashas. The present work by Aešcoly provides the original Ge'ez (classical Ethiopic) text, with facing French translation, a full critical textual apparatus and a literary historical introduction to the longer works. It also contains a general introduction to Falasha literature and an index of the numerous angelic and other magical names which occur in these documents, with suggested etymologies and citations of similar names from the comparable Hebrew, Coptic, Greek and other literatures. (JOSEPH H. GREENBERG)

Time's Arrow and Evolution. HAROLD F. BLUM. (xi, 222 pp., 11 tables, 20 figures, $4.00. Princeton University Press, Princeton, 1951.)

In this important book the author deals with some of the possible relationships between the second law of thermodynamics and organic evolution. More particularly, he is concerned with the nature of the universe and the physical and chemical laws which have channeled the evolution of life in particular directions, and even earlier have inexorably caused life to emerge on this planet. This is not a book which gives a description of organic evolution, or even less, a story of man; but rather it is an account of the physical environment that has set the boundaries and pathways fit for the evolution of life. In this respect it repeats its important predecessor, Henderson's "*The Fitness of the Environment*". It is to the student interested in the origin of life and in a biologist's inquiry into the nature of the universe that this book will appeal. What distinguishes Dr. Blum's approach from some others in this field is the wealth of data and breadth of interest that characterize his discussion. It is highly informative and imaginative, and does not require an expert background in the physical sciences to understand the development of the thesis or the implications involved. (FREDERICK P. THIEME)

The Idea of Conflict. KURT SINGER. (181 pp. 8/6 net. Melbourne University Press, Melbourne, 1949.)

This book is a fascinating and penetrating inquiry into the problem of the differential significance of strife in a number of cultures. Firstly, conflict in the modern West is investigated, and its cultural sources uncovered. Nordic legacies with their "glorification of annihilation" (p. 41) are contraposited to the two Mediterranean realms of the Greco-Roman and the Christian worlds, in both of which militant urges were balanced "with powerful visions of harmony and measure" (p. 48). Relentless conflict with evil characterizes ancient Christian outlook, as well as Iranian creeds, Biblical Judaism, the religion of Ikhounaton, and Islam. The histories of these theistic religions show that "religious wars and persecutions tend to surpass all other inter-human conflict in severity and ruthlessness" (p. 85).

The idea of conflict is then followed back into "archaic culture" and beyond, into the sphere of human origins where, just as among young children of our own civilization, strife and feud are found to figure as major motive forces. Finally, reverting to the psychological and philosophical planes, the inquiry closes on the sombre note of recognizing the necessity for life of the forces of both Eros and Thanatos, of Love and Death, of the two movements, one "which makes" and one "which unmakes"—with the possibility of resolution envisaged only in Utopian terms.

"The Idea of Conflict" paints a significant and broad panoramic picture with sweeping brush-strokes and a few selected colors. The method of inquiry is fruitful; its results can be important for our present strife-torn world. This reviewer feels that a more detailed study by the author of the idea of conflict in the cultures dealt with only too briefly in the book, as well as in cultures omitted or merely touched upon, would be most desirable. (RAPHAEL PATAI)

An Autobiographical Study. SIGMUND FREUD. Authorized Translation by James Strachey. (141 pp., $2.50. W. W. Norton & Co., New York, 1952.)

This work was first published in 1925 as a contribution to a collection of papers designed to give a picture of the present state of medicine as revealed in the autobiographies of its leaders. The present edition contains a postscript dated 1935 in which Freud says, "Two themes run through these pages: the story of my life and the history of psychoanalysis. They are intimately interwoven. This *Autobiographical Study* shows how psychoanalysis came to be the whole content of my life and rightly assumes that no personal experiences of mine are of any interest in comparison to my relations with that science." Written with Freud's characteristic clarity and measured phrasing, this small volume recounts his early work in physiology and brain anatomy, his work with Charcot and hypnosis, and his development of Breuer's undigested experience in one case into the beginnings of psychoanalytic theory. It continues, touching on the development of each major concept of that theory in the context of Freud's own experience. This volume is a personal account of the history of psychoanalysis by its founder. (DAVID M. SCHNEIDER)

'Twixt the Cup and the Lip. MARGARET CUSSLER and MARY L. DE GIVE. (262 pp., 26 pl., $3.95. Twayne Publishers, N. Y., 1952.)

This study of food habits and preferences is based on extensive field work in the rural South conducted by the authors over several years. The opening chapters, however, deal with the general subject and draw illustrative material from ethnographic reports on a variety of peoples and communities over the world. Later chapters describe the results of the southern field work and include documentary photographs as well as excellent tabular presentations of the actual foods and dietary patterns of different rural classes such as landowners, tenants, sharecroppers and wage laborers. The book should be fine source material in teaching sociology, cultural anthropology, social psychology and similar courses. It may be somewhat over-documented, giving it a rather pedantic appearance which is belied by its wholly refreshing and readable style. All in all, a first rate job happily free from either textbook flavor or the arid formality of an orthodox field report. (FORREST CLEMENTS)

NOTE

The list of "Publications Received" has been transferred to the *Bulletin* of the American Anthropological Association, which is now sent to all members. There it will continue to inform readers about current titles and will serve, as in the past, as an acknowledgement to publishers.

Letters to the Editor

Sapir and the Training of Anthropological Linguists

Sir:

In Voegelin and Harris's recent article, "Training in Anthropological Linguistics," a point of history is brought up which is deserving of some further clarification. In particular I have reference to the following statement:

> "While at Chicago, Sapir's graduate students, though engaged in descriptive linguistics, worked in the anthropology department, and this with Sapir's full blessing. At Yale Sapir seemed to reverse himself; he recommended that all work in linguistics be done in the linguistics department, and this despite the fact that he himself was chairman of the Yale department of anthropology." (1952: 324)

Since the authors then attempt to seek the reason for what they call "this seeming reversal in attitude" on the part of Sapir, I should like to point out that, as I remember it, there was no real reversal in Sapir's attitude. What may appear on the surface to have been a change in attitude was brought about by a change in circumstances. In other words, what Sapir did at Yale he would have done at Chicago had circumstances permitted. But in order to clarify this, I should like to summarize what I can remember in the way of pertinent information during the period immediately before and immediately after Sapir's transfer from Chicago to Yale.

During Sapir's sojourn at Chicago, all anthropology students were expected to study some linguistics, a plan which was reflected in Cole's remarks: " . . . when Sapir and I were developing plans for graduate work at Chicago, we were both insistent on training in all branches of the subject" (1952: 162). Students who intended to specialize in ethnology, archeology, or some other non-linguistic branch of anthropology, took most, if not all of their linguistic work under Sapir, and their course work thus remained entirely in the Department of Anthropology. Students who expected to specialize in linguistics, on the other hand, were required to take additional linguistics courses in some other department, e.g., such courses as Sanskrit and Comparative Indo-European in the Department of Comparative Philology, as well as Gothic and frequently also Old High German in the Department of German. Thus a considerable amount of cross-departmental training was insisted upon. This cross-departmental training did not all work in one direction, either, for students in the Department of Comparative Philology were expected to do some work under Sapir, in particular to take his course in phonetics. This requirement, however, met with some opposition, as I recall it, because some students in comparative philology were disposed to feel that the study of nonliterary languages was too bizarre to be taken very seriously.

In the case of an occasional student who might wish to specialize in linguistics to the exclusion of other branches of anthropology, Sapir would have been willing, even while still at Chicago, to permit such a student to take his degree in some department other than anthropology provided the dissertation could still be written on the subject of a primitive, i.e., nonliterary language. However, it was not feasible at Chicago at that time to work out such an arrangement with any other department. The Department of Comparative Philology was exactly and quite properly that, and not a department of linguistics. Therefore the only department in which a doctoral dissertation in primitive linguistics could be appropriately considered was in the Department of

Anthropology. This, then, is the reason why, "While at Chicago, Sapir's graduate students, though engaged in descriptive linguistics, worked in the anthropology department." In this connection it is also important to note that Sapir's title at Chicago (as well as, later, at Yale) was Professor of Anthropology and Linguistics.

Sapir's earlier training was in Germanics and he entered the field of anthropology after being inspired by Boas' unique knowledge of primitive languages. Boas, however, insisted that Sapir study all branches of anthropology, including physical anthropology which he disliked. According to Cole, "this nearly sent Sapir to Indo-Germanic" (1952: 162).

Just as Sapir came to study under Boas after having acquired his interest in linguistics in another department, so an occasional student came to Sapir after having begun linguistic studies in some other department. Sapir, remembering his own dislike of physical anthropology in his student days, no doubt sympathized with such a student's lack of interest in other branches of anthropology. He would therefore have been willing to work out an arrangement of the sort I have mentioned here had it been possible. He remained true to the Boas tradition, however, in that he had no wish to alter the broadly-conceived requirements for a degree in anthropology.

The situation at Yale, on the other hand, was quite different from that which obtained at Chicago. In the year that Sapir went to Yale (1931), the Department of Linguistics was set up, consisting of Sapir himself, and three eminent linguists already at Yale, namely, Professor Franklin Edgerton and the late Professors Eduard Prokosch and Edgar H. Sturtevant, the last-named of whom was the chairman. Sapir was also, of course, the chairman of the Department of Anthropology. Within this framework, then, it was possible for a student interested in working on nonliterary languages to take a degree in either the Department of Anthropology or the Department of Linguistics. And this is exactly what happened. For example, during the early part of Sapir's Yale career, there were four students working towards degrees with the expectation of writing grammars of American Indian languages. Of these, two were enrolled in the Department of Anthropology and two in the Department of Linguistics. The distinction was made, not on the basis of the subject matter of the dissertation, but on the nature of other phases of the students' training. Those whose training was in the field of general anthropology took their degrees in anthropology, while those whose training was in the field of general and historical linguistics took their degrees in linguistics.

The above facts are submitted as a matter of history. The most interesting point is the fact that Sapir would have been willing to institute a disciplinary distinction between linguists and anthropologists *even earlier* than he was able to institute it. And this, I think, is an important point which changes in part the interpretation that Voegelin and Harris have put upon it.

According to their interpretation (pp. 324–325), the change was brought about by the new developments in descriptive linguistics which took a definite upswing in the 1930's. There can be no doubt that Sapir fully appreciated the importance of these developments, since he himself had of course made important contributions to them. But I doubt very much that this was the only reason, or even the chief reason why Sapir wanted to encourage some (not all) of his linguistics students to study more linguistics rather than more anthropology. The principal reason, as I see it, was that Sapir still held to his conviction that since the problems of the historical relationships of American Indian languages were going to have to be solved by Americanists, such

linguists should have sound training in the methods of Indo-European comparative linguistics. After all, training in descriptive linguistics as a part of anthropology was in the Boas tradition; it was training in Indo-European comparative methodology that had to be sought outside anthropology.

There is also another important motivation, which should by no means be overlooked. Sapir was very unhappy about the indifference and even antagonism he encountered among many historical linguists toward the study of nonliterary languages. This antagonism is still to be encountered in some quarters, but it is less virile than it was during his lifetime; and the fact that it is less virile is due in no small measure to his efforts. In the early 1930's, when a group of us were planning a book which was to contain concise but complete grammatical sketches of a wide variety of American Indian languages (Hoijer and others, 1946), he often expressed the feeling that he hoped such a volume would serve to show that the grammatical structures of nonliterary languages are every bit as deserving of serious study as are those of literary languages. He even named a few prominent historical linguists whom he hoped to influence by means of such a volume. This, however, is but one aspect of the battle he waged unceasingly throughout his life against closed frontiers wherever he saw them, whether in linguistics or in anthropology. And in view of this, I cannot help but suspect that one of the reasons why he wanted some of his students to take their degrees in linguistics was that he hoped to "infiltrate," if I may be permitted to use the term in this connection, the ranks of linguists with some who were trained in the techniques and methods of working on nonliterary languages. Therefore it was not that "Sapir felt that linguists were non-anthropological beings who had to work in a frame of reference of their own, and in a department of their own," (p. 324), as Voegelin and Harris surmise. It was not anthropologists who had to be convinced of the value of the study of primitive peoples—in those days it was their bread and butter, but many linguists did have to be convinced. After all, there were linguists before there were "anthropological" linguists. Sapir was not trying to bring about a break between anthropological linguists and other anthropologists—he was trying to promote a better understanding between anthropological linguists and other linguists.

In their concluding paragraph, Voegelin and Harris describe the goal of present day linguistic training in the following terms:

> "The goal includes a structural description of the languages of the world. . . . It includes also the study of the relationship of languages of the world to cultures of the world. For both descriptive and ethnolinguistic work, training is needed that will bring about sophistication in phonemics and in combinatorial linguistics." (p. 327)

This goal is admirably stated as far as it goes. But it includes no mention of historical linguistics, and to this I should like to voice my objection, as I believe Sapir would also voice an objection. To the historically minded linguist, descriptive analyses are necessary before historical studies can be begun. In the field of nonliterary languages they are in fact a *sine qua non*. However, these analyses are not an end in themselves, but a very necessary means to another and more far-reaching end, namely the discovery and validation of broad historical relationships.

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HISTORIANS AND "LAWS" OF HISTORY

SIR:

I should like to add a note to Kenneth Bock's stimulating "Evolution and the Historical Process," (Vol. 54: 486–496). Bock takes sharp issue with what he calls the "traditional view" of historians that history consists of discrete unique happenings from which by their very nature no generalizations can be derived. Historians, says Bock, "see in history 'only one emergency following upon another as wave follows wave, only one great fact with respect to which, since it is unique, there can be no generalization' . . . " (p. 489).

It would be unfortunate if Bock's otherwise admirable discussion were by implication to confirm in the minds of anthropologists a factual misunderstanding of long currency: that most historians subscribe to the credo just quoted. Certainly many do—but many do not. Of the presidents of the American Historical Association whose annual addresses showed any interest at all in the matter, about as many expressed the opinion that broad generalizations could some day be made as expressed the opinion that they could not (Ausubel 1950). A like division of opinion is suggested by the careful equivocation in a recent report of the Committee on Historiography of the Social Science Research Council. After pointing out the difficulties so far encountered in attempts to discover "laws" of history, the committee blandly remarked: "Whether these difficulties can ever be so overcome as to render historical studies as exact as physics is open to question." (1946: 138f)

Bock notes that "the present widespread conviction that history and science look at radically different worlds, with different results, is supported largely by arguments by German scholars advanced in the last century" (p. 493). Here, clearly, he refers to the writings of such men as Eduard Meyer and Ernst Bernheim. While both Meyer and Bernheim elaborately argued this thesis, neither denied that scientific generalizations could be obtained from a study of history. Both men, it is true, denied that laws (*Gesetze*) of universal validity could be so obtained, but both affirmed that rules (*Regeln*) of general if not universal validity could. (They supposed that the "laws" of the natural sciences were such *Gesetze*, but today most natural scientists and philosophers of science consider them mere *Regeln*.)

The point both men urged was that to discover the *Regeln* of history was not the business of historians but of others—of sociologists, said Bernheim (1908: 101–144); of anthropologists, said Meyer (1924 I: 1–67). In short, for what small comfort it may be to them, anthropologists and sociologists who choose to study history comparatively in a search for basic generalizations do so with the full blessing and warm approval of Ernst Bernheim, Eduard Meyer, and a substantial number of presidents of

the American Historical Association. The Committee on Historiography of the Social Science Research Council is less encouraging: it shrugs its shoulders and awaits results.

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KENNETH E. BOCK AND SPACE-TIME

SIR:

Bock's recent article conclusively demonstrates two things: one is the contention of Leslie White that in some areas the Boasian influence has been largely negative; the other is that the outworn physics from which this influence was derived was a very high abstraction.

The Boasian type of empiricism was perhaps excusable when you considered the vast popularity of the scheme of interpretive ideas derived from the first Natural Philosophers; also, if you made it a strict point not to inquire of them how they went about their interrogations of nature. But the time has now arrived when such an empiricism is directly responsible for the tremendous gulf between anthropology and the study of man.

If our general level of confidence is such that this discipline must be erected on the ideas of other people let us at least do so with respect to the more recent contributions, those, say, of fifty years ago. For example, the Einstein-Minkowski assimilation of space and time just might lend itself to our designs. These men found that space and time by themselves were abstractions and that a fundamental reality existed as expressed in the hyphenated form: space-time. Length and time thus became merely ways in which you went about looking at nature (dimensions of coordinate systems); nature herself being better described in terms of intervals rather than inches or seconds.

It follows that history is an abstraction from space-time. It also follows that the Bockians, for all their footnotes, will continue to miss White's main contribution until brought down from the abstract heights of their "historical empiricism."

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SIR:

So scarce are distinguished modern studies of Andean peoples that when such an excellent work as the first volume of Harry Tschopik's *The Aymara of Chicuito, Peru* appears, it deserves even more attention and praise than it received in the review by Weston La Barre (*American Anthropologist* 55: 111–112). I agree with La Barre that the book is a good one; in fact, I think it is of such superior quality that it deserves an additional note of commendation.

Tschopik's study of Aymara magic represents a landmark in Andean studies. Just to have spent two and a half years among the Aymara is in itself a feat of no mean distinction. But more significant is the fact that Tschopik, conducting his investigations at high altitudes and under trying conditions, has produced a study rarely equalled at sea level and under such more favorable circumstances.

Not only does Tschopik's study add a great deal to our knowledge of Aymara culture and personality, as La Barre pointed out, but I know of no better functional study of magic anywhere. If this volume is any indication of what is to come, we may soon expect to have one of the best series of dynamic functional studies on a single group that exists in the anthropological literature.

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Brief Communications

The Todas and Sumeria—A Hypothesis Rejected

In a recent paper Prince Peter of Greece (1951) postulates a new "origin" for the Todas—to be added to the many already put forward and rejected (Scythian, Vedic Aryan, Israelite, Macedonian—to mention only the more fantastic suggestions). He finds in Toda ritual utterances a number of god names which he suggests may possibly be derived from Sumeria. He says specifically (p. 23): "it is not excluded that merchants from Sumeria were established in India for trade purposes, and the Todas may be the only living descendants left of such expatriated buyers." Although historical contact of some sort between Sumerian culture and the remote ancestors of the Todas is something that cannot be either proved or disproved without vastly more evidence than we are likely ever to have, it must be said that progress in Dravidian linguistic studies will provide the only convincing answer to the question as posed in this paper.

During correspondence with Prince Peter on the material that he has presented, I pointed out that it was already possible to find good Dravidian etymologies for a number of the Toda words that he wished to use, and to apply to some others linguistic analysis that dissipated the similarities seen with Sumerian names. Not all of my remarks can be reproduced in a note such as this, but a few may be presented.

One clear and easily stated point is that connection between the Sumerian goddess name Ninlil and the Toda ritual word for the earth is unlikely. This ritual word is identical with the ordinary Toda word neln "ground, earth," a general South Dravidian word in origin—Tamil-Malayalam nilam, Kota nelm, Kannada-Koḍagu (Coorg)-Tulu nela.

The Sumerian goddess Ishtar was found in the first of two ritual words referring to a village long since disappeared, toštaṣ of the clan kaʻṣ. These words (as I recorded them on numerous occasions) are oʻštaṣ, oʻrxwīṛ. The words are compounds containing the same prior member, followed in the first by taṣ "which is above" and in the second by kwīṛ "stream." The prior member has two allomorphs oʻš- and oʻr-, the alternation between the consonants depending probably on the following sounds as in a number of other instances. The prior form is oʻr-, which may conceivably be identical with the word oʻr "way, entrance into thicket, hole" (which, incidentally, has a perfectly sound Dravidian etymology). Analysis of proper names is often uncertain in the final identification of compounded elements. Nevertheless, any surface similarity to the name Ishtar has evaporated.

Prince Peter has recorded ritual words that I did not have opportunity or time to get down during my fieldwork, and not all the material that we have in common will admit of final analysis and Dravidian etymologizing, since the long isolation of the Todas in the Nilgiris has produced the usual linguistic divergences from the parent stock. It is my opinion, however, that further intensive work, both in the field and in comparisons with the other Dravidian languages, will leave so small a residue of unexplained material as to make the possible Sumerian connection untenable.

Intensive comparative work done since the correspondence referred to above, has cleared up the names involved in Prince Peter's first Sumerian connection. He equated the principal Toda deity ön, a god of the dead, with Sumerian An (in Semitized form Anu). One of the ritual names of ön is Iṇṭöw. I had long known that the second element

here is töw "god" (indirectly borrowed from Sanskrit deva- "god"), that the first element is seen also in ïṇo·ṛ "the country of the dead" (which has second element no·ṛ "place, country"), and that the first element in both is ïṇ "the dead," which is cognate with Tamil piṇam, piṇaṙ, Malayalam piṇam, Kota peṇm, Old Kannada peṇa, Tulu puṇa, Telugu pīnūgu, pīngu, Kuwi (Fitzgerald) pīlingū, all meaning "corpse, dead body." Recently the etymology of ö·n has been found: Malayalam pēna "ghost, spirit," Kota pe·n, pe·nm "possession of woman by dead ancestors," Old Kannada pēṅkuṇi, pēṅkuḷi "demon," Gondi pēn "a god," Kui pēnu "a god, a spirit," Kuwi (Fitzgerald) pēnū "god," (Schulze) pēnu "devil." In addition one must connect the following words which have different suffixes: Tamil pēy "devil, goblin, fiend," pēycci, pēcci "demoness, woman under possession of a demon," Malayalam pē "demon," feminine pēcci, Kota pe·y "demon," Tulu pēyi "demon," and Old Kannada pētu, Middle Kannada hēde "demon."

The further implications of this family of etyma are fascinating, but still not quite certain. Have we found here the proto-Dravidian morpheme for "god"? Certainly, the fact that two central Indian languages (Gondi and Kui-Kuwi) have this meaning looks in that direction. If so, the meaning has undergone pejorative change in the southern languages that preserve the morpheme (Tamil-Malayalam-Kota pēy, Kannada, Tulu) under the pressure of borrowed Indo-Aryan words for "god" (chiefly Sanskrit deva- and its derivatives); Schulze's meaning "devil" for Kuwi may have been dictated by his missionary interests. The southern meaning has an exception in the Nilgiri languages Toda and Kota, that connect the words that have the -n- suffix with the realm of the dead: Toda ö·n "the god of the dead" and Kota pe·n, pe·nm "possession of woman by dead ancestors" (probably partial independent development); the Malayalam meaning "ghost" for pēna cannot be checked further at present, but may be an indication of Malabar-Nilgiri connections.

Both the Toda words ïṇ and ö·n have lost initial p of proto-Dravidian, which is not a regular development in Toda, but is regular in modern Kannada, the language of the plains to the north of the Nilgiris, and in the Kannada dialect spoken by the Badagas in the Nilgiris. The Todas may have undergone influence of this sort at any time in their long history in the Nilgiris, and only further study (perhaps fieldwork on the Badaga language) will settle this point of relative chronology.

To conclude, linguistic analysis, both descriptive and historical, makes or will in the long run make the major part of Prince Peter's evidence unconvincing by providing the Toda words with general Dravidian cognates. He has rightly put forward his suggestion of Sumerian connections very tentatively, but even so it cannot be accepted. I regret that my long incubation of my Toda fieldnotes has contributed in part to the possibility of making the suggestion, and can only hope that the near future will see this situation remedied.

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On the Nature of Myth: An Analysis of Some Recent Criticism

The recent challenging paper by Gotesky (1952) raises a series of significant questions about the nature and role of myth. First proposing only to "clarify apparent confusions" in an earlier paper by Bidney (1950), it makes a vigorous critique of the position expressed in that paper, then goes on to state its own theory of myth. We believe both the critique and the theory need further analysis.

In his 1950 paper Bidney first considered the treatment of myth by Tylor, Wundt, Langer, Malinowski, and Boas, then briefly formulated his own view, a sort of prolegomena to a general theory. Gotesky does not question the historical analysis, but does attack the theory. Not only has Bidney unwittingly tried to hold in solution six destructive contradictions, but, so Gotesky charges, he also uses myth as false belief in three different senses; these errors are said to derive from inadequate analysis of the grounds that establish the truth of beliefs, coupled with a conception of scientific knowledge that is anthropologically untenable. Yet if Gotesky does show some of the limitations in Bidney's brief presentation, he also seriously misconstrues it. Since the real differences between the two approaches are of great importance, we hope to clear away the misinterpretations made by Gotesky, then restate the central issue.

I

Let us consider the six contradictions which Gotesky alleges.

(1) Bidney is said to hold that every society distinguishes between myth and nonmyth, and also that myth is not known as such, but believed true. Gotesky asks: "How then, can a society, if it does not know its own myth as myth, distinguish clearly between both?" (Gotesky: 524) Now the original article stated that

> a myth is evaluated as such *only from the perspective of those who do not share the ideas and beliefs under consideration.* (Emphasis ours) From the perspective of those who accept the ideas and beliefs as true or valid, the latter are not myths at all (Bidney: 22).

In its most general sense, then, myth is a term of evaluation, used when something is (1) seriously believed by another, but (2) disbelieved by oneself. In accounting for such evaluations, Bidney finds that they may have two kinds of bases, the one relative and the other objective (p. 23). On the one hand, beliefs are often evaluated as myth by criteria which are only relative to the individuals or the socio-cultural context involved. For example, the Greek body of narratives concerning a former period of epic heroes and earthly adventures of the gods was accepted by some early chroniclers such as Hecateus and Pherecydes; while they modified these narratives according to personal criteria of credibility, for them the accounts were not myth. These accounts, however, were rejected by Xenophanes; for him they were myth, from the perspective of his own belief in a single god "neither in form like unto mortals nor in thought" (Burnet 1920: 121).

On the other hand, some beliefs, such as that cultural superiority is biologically inherited, can be evaluated as myth because they are demonstrably false. Here the criteria are objective, that is, independent of individual or cultural prejudice, being based on the cumulative, self-correcting nature of science.

Returning to Gotesky's question, it is elementary that a group, say the Navaho, may distinguish, relative to its own criteria, belief which it considers valid (for it, non-

myth) from belief which it considers false (for it, myth). At the same time, its own seriously-held beliefs may be judged myth by others, relative to their criteria. Gotesky's phrase, "its own myth," could refer then either to what the Navaho's own criteria judge false, or to what their criteria accept, when this is judged false by the criteria of others; *but not to both.* The myths which the Navaho do not know as such are those which are not myth by their own criteria, but myth only by the criteria of others. This follows clearly from the conditions established for the use of the term "myth." Gotesky's question overlooks these conditions.

(2) Bidney is said to hold that "To know myth as myth . . . occurs at a later time in the history of a given society or in another society" (Gotesky: 524). When recognized as myth, it is no longer believed, for it is now known to be false. Such a belief, Gotesky contends, can not properly be called "myth" by Bidney, since his definition requires that a myth be believed to be true. The point is that it *was* believed to be true; the conditions for evaluating a belief as myth may be satisfied diachronically as well as synchronically. We may speak of the myths of the Greeks, because they *were* seriously believed, even though no offerings now rise to Zeus. Moreover, beliefs held true by an individual may be considered myth later by the same individual (witness Whitaker Chambers), or by contemporaries either of his own society or another, or by members of his own or another society at a later date. All these possibilities follow from the basic definition; it does not imply the exclusive interpretation that myth is known as such only at a later period or in another society (Gotesky: 526–527). Our example from the Greek chroniclers shows this, for Xenophanes in the 6th century B.C. evaluated as myth what Pherecydes accepted in the 5th. More strikingly, when at Warm Springs Reservation an elderly Sahaptin chief narrated his belief about the origin of the tribe, the middleaged Sahaptin interpreter made clear he considered the account a myth by as cursory as possible a translation.

(3) Bidney is said to hold that myth cannot be beyond truth or falsity, yet at the same time approve Malinowski's thesis that myth is belief which is not scientifically substantiable, and intended primarily to justify cultural beliefs and practices. The text in question (Bidney: 19) shows that the discussion of Malinowski is simple exposition; no approval is expressed. Since the paper is directed against those views which define myth primarily in terms of social function, there are no grounds for the interpretation that it approves such a view in Malinowski.

(4) Bidney is said to imply that scientific criteria are the only means for distinguishing myth; since scientific criteria did not exist in pre-scientific societies, it follows, Gotesky contends, that no pre-scientific society could distinguish myth. On the contrary, two kinds of criteria are expounded in the original paper, as was pointed out under (1) above. Gotesky notices only the objective type (p. 525), and this error vitiates the bulk of his critique. The original paper does state that scientific criteria are the only objective, normative means for distinguishing myth; they offer a means of evaluating belief which is trans-culturally valid. Belief in biologically inherited Jewish inferiority is myth, and this evaluation of it is not simply relative to a particular culture at a particular time but objectively true; nor was the contrasting evaluation made by Fascists simply relative to their particular cultural background, but objectively false. But the emergence of science is not necessary in order for beliefs to be evaluated by relative criteria.

(5) Since Bidney states that science generates its own myths, it is held to follow

that science can not, as he claims, guarantee that myth will be known as such. The passage in question is

> Science, in turn, gives rise to its own crop of secular myths, since science, as a self-correcting quest for knowledge, tends to outmode some of its own concepts and theories (p. 25).

That science may generate social myth is clear from the history of social Darwinism; a study of the belief of earlier scientists in the Newtonian approach makes a similar point, terming it a myth within science (Santillana 1941). Yet science generates also a great number of truths, and eliminates far more myths than it inspires. Is it to be thought a contradiction that many beliefs are revealed to be myth by science, as cumulative, while at the same time some scientific beliefs are later rejected, judged mythical, by science as self-correcting? Contesting the majority (Neo-Darwinian) viewpoint in genetics on macroevolution, Goldschmidt writes:

> The future will bring a decision. It should be emphasized again, however, that this is only a very special problem, while the major facts and explanations of evolution by mutation, selection, and adaptation are established forever (1952: 98).

Science cannot guarantee that *all* myth will be known as such, now or in the future; it can guarantee, so long as it can proceed critically and independently, that more and more myth will be so known.

(6) If science can distinguish myth, as Bidney asserts, it is held that myth must disappear, so that the more scientific an age, the less myth it contains; it is held that this "completely contradicts Bidney's basic thesis that every culture creates its own type of myth" (Gotesky: 525). The discussion of (5) covers much of this alleged contradiction. To assert that science reduces the amount of myth does not imply that its task will someday be complete, that a point will be reached when all old myths have disappeared and no new myths emerge. The point is that the "process of myth-making does not die a natural death with the emergence of critical, scientific thought" (Bidney: 26). Witness Mussolini's boast, "We have created our myth" (in Sabine: 759). Lag in the communication of scientific results to the society as a whole, and the exploitation of scientific results for non-scientific ends increase the danger. Indeed, the readier we are to base our sciences on too narrow a conception, the more likely we are to generate or abet myth, even in the name of fighting myth and of being scientific. Hockett cautions that it is

> a serious mistake to react to doctrinaire tendencies by developing equally doctrinaire tendencies in another direction; to reject the accomplishments of our predecessors wholesale without painstaking individual check; or to try to draw rigid lines of demarcation between the astronomy and the astrology of our field of investigation at as early a date as the present (p. 57).

Though written about linguistics, this describes mistakes which may occur in any field. Regarding the relation of society and myth, it is especially pertinent to that positivistic approach which declares the judgment of social values and goals to be beyond the scope of science. Such a divorce, rather than preserve the purity of science, can only nourish the makers of myths.

Besides these six misinterpretations, there are others. For example, Gotesky writes that

> In one place, Bidney gives the word "myth" a meaning entirely different from the senses employed

above, its aesthetic use as symbol, by poets and writers, of the value-charged beliefs (myths) of previous cultures. It is obvious that these "myths" are not myths to these poets and writers. They do not believe in their truth (p. 525).

On the contrary, these are myths to such poets and writers precisely because they do not believe in their literal truth, according to Bidney's view. At the same time, Bidney recognizes that these myths may be said to contain a degree of symbolic truth. Amusingly enough, they may be myths by Gotesky's approach too, in so far as poets and writers believe in this symbolic truth; and a large body of literary criticism takes exactly this point of view.

In sum, the triumphant semantics do not clarify, but confuse. In proliferating alleged contradictions and ambiguities, they fail to detect the two basic points: the relative and objective evaluation of belief as myth, and the dual conditions for making of such evaluation. This is the less understandable in that the latter point is stated rather accurately as item (iii) of a brief summary of Bidney's view (Gotesky: 526). Inconsistencies have been tortured out of a presentation that is consistent and intelligible, if read without animus and with regard for context. If some inadequacies of exposition are found when a comprehensive subject is treated in such brief compass, there is still no warrant for the distortions placed upon the text.

II

Since the rest of the critique presupposes the errors of interpretation already discussed, we need not examine it in detail. One further point, however, should be made. The critique seems to attribute to Bidney a simple dichotomy between myth, the non-scientific, and non-myth, the scientific. Examples are given of types of belief which might reasonably be called myth, because they are believed and value-charged, but which are not accounted for by the alleged requirement of being scientifically proven false. Our previous exposition shows that they might easily be treated as myth, relative to some philosophical, religious, or other perspective which considered them false. Bidney does not impose a moiety system upon beliefs. Three categories are indicated in the passage, "so far will responsible and mature minds prefer objective truth and well-founded faith to fictions and myths" (p. 24). This suggests that between the poles of scientific truth and scientifically-proven myth there is a third area of belief, where scientific proof cannot be invoked. If beliefs in this area are evaluated as myth or truth, it is by criteria which are relative to a philosophical system, religious creed, moral preference, or other hypothetical truth, which goes beyond the empirical knowledge of the time. The relativity of choice in this area of socially and historically relative truth need not imply the equal validity of all choices. If there may be "well-founded faith," there may be ill-founded faith as well. And given a hypothetical belief,

> The scientist hopes to test the results experimentally or experientially, and (at least in theory) has no hesitation about rejecting any hypothesis whatsoever which fails to meet those tests. The non-scientist may reject some portion of experience, or even avoid any search for it, in order to be comfortable in the retention of hypotheses which are attractive for some personal or cultural reason (Hockett: 54).

The more the former attitude prevails, the more of this middle category that will be apportioned to the two poles.

III

Gotesky clearly recognizes the middle area of belief, neither scientifically false nor true, and it is a valuable contribution of his article to delineate it in a way that Bidney did not. For Gotesky, however, the beliefs in this area are to be termed myth, when so defined "by employing the negative canons or conditions of scientific belief" (p. 531). Such beliefs are to be called myth because (1) they transcend all possible scientific truth criteria, or (2) though theoretically in conformance with truth criteria, they are not practicably testable at present, or (3) they transcend present truth criteria, though they may be tested later, or (4) they serve as basic postulates of a social system to integrate human actions and define its procedures. Myth is beyond truth and falsity in this view, since the truth or falsity of a belief is irrelevant to its classification as myth. The necessary and sufficient ingredient of myth is its particular social function. This seems implied in the passage

> Every culture will create and value its own myths, not because it may not be able to distinguish between truth and falsity, but because their function is to maintain and preserve a culture against disruption and destruction (p. 530).

In sharp contrast to this is Bidney's insistence that

> Insofar as it is acknowledged that there is a normative body of well-authenticated truth, and that human reason is capable of distinguishing truth from falsity and fiction, so far will responsible and mature minds prefer objective truth and well-founded faith to fictions and myths, regardless of the latter's so-called pragmatic value. To regard myth as a neutral term beyond truth and falsity, and to interpret the culture of scientific rationalism *as if* it also were based on myth, is to undermine the very basis of rational and scientific thought (p. 24).

The fundamental issue, then, is whether myth is to be defined essentially in terms of truth and falsity, or in terms of pragmatic social value. It is an unresolved one in anthropology. The anthropologist's typical usage is not, as Gotesky's characterization of it might suggest, wholly on his side. While "myth" originally had no negative connotation, simply designating a narrative, it soon acquired a negative sense. In European culture this sense dominated the eighteenth century movement of Enlightenment, and it remains largely true in scientific writing that

> myth is not a scientifically neutral term; on the contrary, it is a value-charged term and implies a negative evaluation (Bidney: 22).

Herskovits, for example, referring to one traditional body of narrative and belief, condemns it as the *Myth of the Negro Past*. M. F. Ashley-Montagu has written of *Man's Most Dangerous Myth: The Fallacy of Race*.

The issue is not simply a terminological one. Underlying it are fundamentally opposed conceptions of scientific knowledge, the social role of science, and the nature of cultural ideology. Gotesky's paper performs the service of bringing these to the fore. While we cannot analyze it here, we may remark that Gotesky's extreme conception of the relativity of scientific knowledge leads to a situation in which not only is "the determination of the mythical nature of beliefs difficult, if not impossible, for the anthropologist" (Gotesky: 530), but so is the practice of anthropology as a whole. A number of anthropologists felt scientifically justified in declaring the racist beliefs of

Nazi Germany to be myth because objectively false. From Gotesky's reservations about the relativity of "truth criteria," it would appear they were mistaken.

To define myth primarily as false belief, and to point to the disruption and destruction it may involve, as in Nazi Germany and Fascist Italy, does not imply any lack of appreciation for the role and value it may have in other cases, as shown for the Navaho by Kluckhohn (1942: 59–78). But the full implications of either approach are outside our scope.

That myth, as oral narrative, should be a primary concern of anthropologists was documented a few years ago by Hallowell (1947). Earlier, Kluckhohn (1942) argued as a general thesis that myth, as "sacred tale," together with ritual had a vital adaptive and adjustive role. These two more recent papers by Bidney and Gotesky, extending discussion of myth to basic questions of cultural ideology and scientific knowledge, show further reason for giving the concept of myth widespread attention in anthropology. This is the more desirable and appropriate because anthropology controls much of the basic data.

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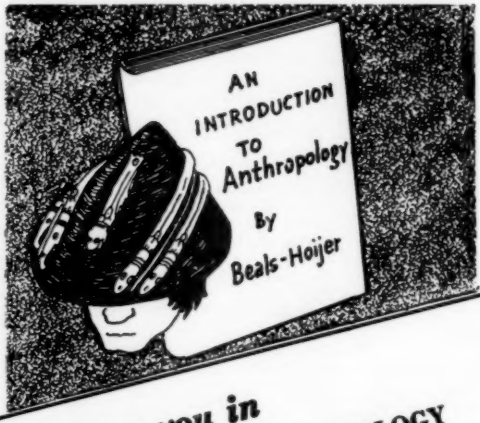

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